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BIOCHEMISTRY.—*The chemical nature of enzymes.*[1]

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Why was it difficult to isolate an enzyme? Here I employ the word "isolate" as meaning preparing in pure condition. It does not seem difficult to isolate and crystallize an enzyme now, but it was difficult 20 years ago. The reasons were several. One was the inertia of men's ideas. Another reason was the influence exerted by Willstätter and his school, who held that enzymes were neither lipids, carbohydrates, nor proteins and who believed enzymes to exist in excessively low concentrations in plants and animals. The misconception that colloidal substances had a chemistry different from that of crystalloids was another stumbling block. A yet more important reason why enzymes could not be isolated readily was because work with these substances requires a special technique. The enzyme is present in relatively low concentration in the raw material. It often is highly unstable. The raw material nearly always contains natural protectors which are left behind during the purification processes. In the absence of these natural protectors, the enzyme may become destroyed by traces of heavy metals, by oxidation, by unfavorable pH, or even by autolysis.

I wish to tell next why I decided in 1917 to attempt to isolate an enzyme. At that time I had little time for research, not much apparatus, research money, or assistance. I desired to accomplish something of real importance. In other words, I decided to take a "long shot." A number of persons advised me that my attempt to isolate an enzyme was foolish, but this advice made me feel all the more certain that if successful the quest would be worth while.

The reasons why I chose to work with urease were several. I had been working with urease as a reagent for the estimation of urea in muscle, blood, and urine. This urease was prepared from soy bean meal. In 1916, Mateer and Marshall found that the jack bean, *Canavalia ensiformis*, contained about 16-fold more urease than the soy bean. The jack bean appeared to me to be extraordinarily rich in urease and I could see no reason why this enzyme could not be isolated in pure form and characterized chemically. Claude Bernard has said that success or failure may depend upon the lucky choice of some reagent or raw material. Willstätter was unfortunate in his choice of saccharase as an enzyme to isolate. I was fortunate in choosing urease.

I hoped that urease would turn out to be a globulin, since globulins usually, if not always, are present in beans and seeds, and since globulins are easy to precipitate by dialysis. Other reasons for choosing urease as the enzyme to isolate were that this enzyme can be estimated quantitatively very accurately and readily, and that urease could be expected to be one enzyme, rather than a mixture of enzymes, acting as it does on such a simple substrate as urea.

I started trying to isolate urease in the fall of 1917, having been occupied previously with analytical methods. At this time, our laboratory contained no adequate apparatus for grinding jack beans. We first used a coffee mill and then ground the coarse material with a mortar and pestle. Years later, we constructed a mill which was run by an electric motor and which

[1] Nobel Laureate Lecture delivered at Stockholm, Sweden, December 12, 1946. Previously published by the Royal Academy of Sciences, Stockholm.

employed a bolting device, However, in the meanwhile we usually used commercial jack bean meal. This commercial meal was not always satisfactory.

The jack bean is a miniature world in itself and contains all of the elements required for life, growth, and reproduction. I decided to isolate and characterize as many as possible of the chemical compounds present in the bean. I found various minerals, proteins, carbohydrates, lipids, extractives, pigments, and enzymes to be present. Particular attention was paid to the proteins of the jack bean, since I expected to find that urease was one of these. I isolated two globulins in crystalline form and named these concanavalin A and concanavalin B. A third globulin which I called "canavilin" separated as spheroids upon dialysis. Years later Howell and I were able to crystallize this, after a preliminary digestion with trypsin. Many years later we discovered that concanavalin A is a hemagglutinin for the red cells of certain animal species as well as an excellent precipitant for certain polysaccharides. A lipid fraction of the jack bean was observed to function as a thromboplastic agent.

In attempting to concentrate and purify urease, I employed fractional precipitation with alcohol, acetone, and other organic solvents. Fractional precipitation with ammonium sulfate, magnesium sulfate, and other neutral salts was tried. I tested a large number of salts of heavy metals or precipitants. I employed a very large number of reagents as absorbents. This work covered many years. At times I grew discouraged and temporarily abandoned the quest, but always returned to it again.

At first we used to extract urease from jack bean meal with water. These aqueous extracts were viscous and therefore very difficult to filter. Glycerol extracts were even more bothersome. I learned that Folin used 30 percent alcoholic extracts of jack bean meal as a source of urease for analytical purposes. It was found that extraction with 30 percent alcohol was of distinct advantage, inasmuch as this solvent dissolved most of the urease but failed to dissolve a rather large quantity of the other proteins. Hence, a considerable purification was achieved through the use of this solvent. The alcoholic extracts filtered very rapidly, leaving the undissolved material behind on the filter paper. The only disadvantage of 30 percent alcohol lay in the slow inactivating action of this solvent upon the urease. However, if kept at low temperatures, there was no inactivation of the enzyme.

When kept at low temperatures, 30 percent alcoholic extracts of jack bean meal formed precipitates. These precipitates contained practically all of the urease, together with concanavalin A, concanavalin B, and other proteins. At this time we had no ice chest in our laboratory and we used to place cylinders of 30 percent alcoholic extracts on our window ledges and pray for cold weather.

It seemed to me of interest to employ dilute acetone instead of 30 percent alcohol and to see whether this substitution would result in any improvement in the method of purification. Accordingly I diluted 316 cc of pure acetone to 1,000 cc and used this as the means of extracting the urease. I routinely employed this dilution of acetone, since I had been preparing 30 percent alcohol through diluting 95 percent alcohol in this manner. The acetone extract was chilled in our newly acquired ice chest overnight. The next morning I examined the filtrate. It contained practically no precipitate, thus differing from alcoholic filtrates. However, upon observing a drop of the liquid under the microscope, it was seen to contain many tiny crystals. These were of a shape that I had never observed previously. I centrifuged off some of the crystals and observed that they dissolved readily in water. I then tested this water solution. It gave tests for protein and possessed a very high urease activity. I then telephoned to my wife, "I have crystallized the first enzyme."

Now I should like to tell this audience what enzyme crystals look like. This description applies also to proteins, since enzymes are proteins. Enzyme crystals are nearly always microscopic, since these compounds, being of high molecular weight, diffuse relatively slowly and therefore crystallize slowly. Enzyme crystals belong

nearly always to the isometric or hexagonal systems. Sometimes enzymes separate from solution as spheroids. This formation indicates a tendency to crystallize, as Dr. Northrop can tell you from his experience. Spheroids are nearer the crystalline state than purely amorphous material, such, for example, as casein which has been precipitated from milk by the addition of acetic acid. Spheroids have sometimes been found by us to be aggregations of many needles either in parallel or concentrically arranged.

Having convinced myself that I really had isolated urease in crystalline form, I read a paper on this matter at Clifton Springs, N. Y., and published an article in the August number of the Journal of Biological Chemistry for the year 1926. But now a difficulty arose. The commercial jack bean meal which we had been using suffered a decline in quality and we could obtain no urease crystals from it unless we added a small amount of acetic acid to the alcoholic filtrates. Even then, the yield of crystals was low. Analyses showed that the recent samples of jack bean meal contained only about one-half as much urease as the earlier samples. Accordingly, attempts were made to obtain satisfactory meal, or satisfactory jack beans. We grew jack beans in one of the Cornell greenhouses. The beans grew poorly and the yield was less than the number of beans planted. We obtained jack beans from Texas, Guatemala, and Honduras, but these were low in urease. I happened to meet a plant physiologist, Dr. Albert Muller, who said he would grow me some jack beans at Mayagüez, Puerto Rico. I gave him about a kilo of jack beans rich in urease, the last I had left. Some seven months later about a bushel of beans arrived from Puerto Rico. These beans were rich in urease. The finely ground meal gave a high yield of urease crystals. Later we were able to obtain excellent jack beans from an Arkansas farmer. He has supplied us with jack beans ever since.

I wish to speak now about proofs of the identity of the crystals with urease. In cases of this sort, one piece of experimental evidence is not sufficient to constitute a valid and satisfactory proof; one must submit many pieces of evidence. At this time I had no access to the ultracentrifuge of Svedberg nor to the electrophoresis apparatus of Tiselius. However, I was able to offer evidence of another sort, as is shown below:

1. When the crystals separated there occurred a great increase in purity, namely from 700 to 1,400-fold. Using other methods the increase in purity observed was very much less than this and at times there even occurred a decrease in purity.

2. When the distribution of urease was followed quantitatively it was found that of the urease passing into the filtrate as much as 40 percent or more separated with the crystals.

3. Recrystallization increased the purity of the urease.

4. Adding traces of poisons, such as mercuric chloride or formaldehyde, to jack bean meal inactivated the urease and, at the same time, prevented the appearance of the crystals.

In obtaining the crystals, I felt much the same as a person does who is trying vainly to place in position a piece of a machine. Suddenly the piece slides in as if covered with butter. One knows that it is now where it belongs.

During later work on crystalline urease, I was fortunate to have a number of excellent men in our laboratory. These were Doctors Hand, Kirk, Poland, and Dounce. We found that trypsin neither digested nor inactivated urease. Since trypsin is a proteolytic enzyme of second attack, it does not digest certain native proteins readily. Some proteins that are digested with great difficulty are hemoglobin, ovalbumin, and the serum proteins. After urease had been denatured by acid or by heating, it was found to be very readily digested by trypsin.

Pepsin acts best in a strongly acid medium and strong acid rapidly destroys urease. However, at pH 4.3 we found that urease was so slowly destroyed that it was possible to demonstrate a parallel digestion and inactivation by pepsin. In place of pepsin it was possible to use papaincysteine.

In our laboratory Dr. J. Stanley Kirk was able to immunize rabbits to crystalline urease. Kirk started by giving rabbits as little as 0.03 mg of crystalline urease intra-

peritoneally. This dose was given twice weekly and was finally increased to 1,000 lethal doses. The immunized rabbits contained antiurease in their blood serum. This antiurease could be purified by precipitating it by adding urease, washing the urease-antiurease precipitate, decomposing the complex with 0.05 N hydrochloric acid, bringing to pH 5.0, and centrifuging down the denatured urease. The antiurease was not harmed by this treatment and could be employed as an excellent precipitant in testing for urease. It gave a visible precipitate with solutions of urease diluted 1 to 500,000. However, urease allowed to stand for a few seconds with 0.05 N hydrochloric acid and then neutralized gave no precipitate with antiurease; neither did it possess any urease activity.

Northrop has made good use of physical methods to demonstrate the identity of pepsin with his pepsin crystals. While we have not employed such methods, Kubowitz and Haas, working in Warburg's laboratory, have demonstrated that ultraviolet light is absorbed by highly dilute solutions of crystalline urease and that exactly the same wavelengths that are absorbed are those which destroy urease.

In 1930, Northrop of the Rockefeller Institute obtained pepsin in crystalline form. A short time later, Northrop and Kunitz obtained crystalline trypsin, crystalline chymotrypsin, and also the zymogens of these enzymes in crystalline form. Their monumental work was of very great help in bringing the scientific world to admit that enzymes can be isolated in pure and crystalline condition. In this connection I wish to note that Professor von Euler aided me greatly when I worked on urease in his laboratory at Stockholm's Högskule in 1929 and that I received valuable help in 1937 while working in the laboratory of Prof. Thé Svedberg at the University of Upsala.

The announcement of the crystallization of urease and pepsin was not accepted by some biological chemists. In Germany students of Willstätter attempted to show that our crystalline proteins were merely carriers of the enzymes. It suffices to say that these attempts to disprove our work failed, as they were bound to fail, since Northrop and I were right and since our evidence was unassailable.

To date about thirty enzymes have been obtained in crystalline and in presumably pure condition. Certain enzymes are colored, being combinations of specific proteins with such prosthetic groups as riboflavin, phosphate, or hematin. Theorell has crystallized the yellow enzyme of Warburg and Christian and, remarkably enough, has been able to split the protein from the riboflavin phosphate and later to reunite these two components. He has shown conclusively that the union of these components proceeds stoichiometrically.

The enzymes catalase and peroxidase are compounds of protein with hematin and these two enzymes possess characteristic absorption bands. This property has greatly facilitated the crystallization of these two enzymes. In addition, it should be noted that these two enzymes have been shown to form compounds with hydrogen sulfide, sodium fluoride, sodium azide, etc.

All enzymes are proteins but not all proteins are enzymes. Many, if not all enzymes can be crystallized. The oxidizing enzymes all appear to be conjugated, or compound, proteins, while the hydrolytic enzymes are, as far as we can tell, without prosthetic groups. However, even hydrolytic enzymes must have reactive groups. Every enzyme requires a specific method for its purification. Our present methods of purifying both enzymes and proteins are crude and unsatisfactory for the most part. It is fairly certain that better methods will be discovered in the near future. One can purify an enzyme either by precipitating the impurities or else by precipitating the enzyme. The latter procedure is to be preferred, but the former procedure often is necessary at the beginning. When I speak of precipitating the enzyme I mean, of course, a more or less specific precipitation and not a general precipitation of everything, much as occurs on adding a great excess of alcohol or acetone.

Some day every enzyme in living matter will have been discovered and described. Every chemical reaction which goes on will have been recorded. We probably can ex-

pect to find some enzymes which are glycoproteins, others which are lipoproteins, and others which are nucleoproteins.

In 1917, the role played by enzymes was only partially understood. Digestive enzymes were well known, autolytic and oxidative enzymes were somewhat known, but not well understood. At present we realize the tremendous complexity of the cell. In muscle alone, some 60 enzymes are known to occur. Thanks to relatively recent investigation, practically all of the complicated reactions involved in the breaking down of glycogen to carbon dioxide and water have been made clear.

The organic chemist has never been able to synthesize cane sugar, but, by using enzymes, the biological chemist can synthesize not only cane sugar, but also gum dextran, gum levan, starch, and glycogen.

We know now of the existence of enzymes which employ phosphoric acid instead of water and which might be given the general term "phosphorases." There are the phosphorylases, transphosphorylases, phosphoisomerases, phosphomutases, and phosphodismutases.

From the work of Cori and his associates, we now have evidence that hormones function through their effect on enzymes. Thus, glucose is transformed into glucose-6-phosphate when it reacts with ATP in the presence of the enzyme known as hexokinase. This reaction, essential for utilization of glucose, is inhibited by the diabetogenic hormone coming from the anterior pituitary. This inhibiting action is abolished by insulin.

We can sum up by saying that, as the result of discoveries in the field of enzyme chemistry, some questions have been answered and many new questions have arisen. We live in an expanding universe in more senses than that of the astronomers.

In conclusion, I wish to pay tribute to my former teacher, Otto Folin, who emigrated as a boy of 17 from Småland to America and who, as professor of biochemistry at Harvard, inspired me as he did many other young men.

CHEMISTRY.—*Dr. Stephen Brunauer's contributions in the field of adsorption.*[1]

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(Communicated by JAMES I. HOFFMAN.)

At the outset I think it is desirable to distinguish clearly between physical adsorption and chemisorption, and with that in view I have tabulated below a number of characteristics of each type of process:

Physical Adsorption	*Chemisorption*
1. Rapid	Rapid or slow
2. Reversible	Often irreversible
3. Low heat	High heat
4. Non-specific	Specific
5. Multilayer	Monolayer only

Since this comparison should perhaps be discussed in some more detail, attention is called to the following:

1. Physical adsorption is generally a rapid process whenever the surface is readily accessible to the gas or vapor being adsorbed, although one may encounter a slow adsorption when it is necessary for the adsorbate to diffuse into cracks or capillaries to reach the adsorbing surface. Chemisorption, on the other hand, like chemical reactions in general, may be rapid or slow, and frequently displays a temperature coefficient requiring an activation energy of the order of magnitude found in chemical reactions.

2. Physical adsorption is in general reversible, and it is possible to remove the adsorbate from the surface by outgassing at the temperature of adsorption or at slightly elevated temperatures. Chemisorption, on the other hand, may be truly irreversible, indeed to such an extent that the adsorbate can be removed only by chemical action at elevated temperatures. For instance, oxygen chemisorbed on tungsten can be removed only by heating to about 2000°C. Other chemisorbed layers, however, can be removed by less drastic treatment, let us say by outgassing at

[1] Address delivered before the Chemical Society of Washington, March 1946, on the occasion of the awarding of the Society's Hillebrand Prize to Dr. Stephen Brunauer. Dr. Beebe's preliminary congratulatory remarks are here omitted. Received September 24, 1947.

temperatures of a few hundred degrees above that at which the adsorption process takes place.

3. In general, the heat of adsorption which indicates the energy of binding of the adsorbate molecules to the solid surface is of a low order of magnitude in the case of physical adsorption, usually two to three times the heat of vaporization for the first portions of adsorbate added to the surface. This means that for such gases as nitrogen, oxygen, argon, or carbon monoxide, all of which boil at temperatures approximately that of liquid air, the heat of adsorption will not exceed 5 kcals per mole. With larger molecules, such as butane, we encounter higher heats of physical adsorption, which, however, do not exceed 15 to 16 kcals per mole. Again one finds a marked difference in chemisorption, the heats of this process, like those of ordinary chemical reactions, running as high as 100 kcals per mole in certain instances.

4. Although, as is pointed out in Dr. Brunauer's book (*1*), there has never been given an exact definition of the term *specificity* as applied to adsorption, yet this term is of some use in a qualitative sense. It may be said that in the case of physical adsorption the heats evolved tend to be nonspecific to the chemical nature of the gases or vapors involved if these adsorbates have molecular weights, and therefore heats of vaporization, of the same order of magnitude. For instance, one finds that the four substances mentioned above have heats of adsorption of the same order of magnitude. However, we shall see in a later portion of this paper that we must not generalize too far about this nonspecificity of physical adsorption, and that, indeed, significant differences become apparent especially when we study the physical adsorption of the same adsorbate on different surfaces. As might be expected, the heat of adsorption in chemical processes is highly specific and may vary all the way from, let us say, 15 kcals to 100 kcals in different instances. In this respect, of course, chemisorption again resembles chemical reactions in general.

5. It has been well established by the work of Dr. Brunauer and others that the forces which give rise to chemisorption extend to a negligible extent beyond the monolayer of adsorbed molecules. On the other hand the work of the group with which Dr. Brunauer has been associated seems to establish beyond reasonable doubt that we are concerned in physical adsorption with the formation of multilayers of adsorbate on the solid surface (*2*).

With this background, we are now prepared to turn to a discussion and review of of some of the significant work to which Dr. Brunauer has contributed. In making a selection from Dr. Brunauer's experiments, I have chosen the work that has been especially helpful to my own investigations.

In Fig. 1 are shown the isotherms obtained by Emmett and Brunauer (*3*) for the adsorption of carbon monoxide on two iron catalysts at the temperature of liquid oxygen. Looking first at the isotherms for the pure iron catalyst, it is evident that the curve labeled "total adsorption" lies very much above the "physical adsorption" curve. For reasons which have been abundantly established by the group with which Dr. Brunauer has been associated, the points labeled B on each curve represent approximately the completion of the statistical monolayer of physically adsorbed gas. Thus the volume indicated by Point B in the "physical adsorption" curve is the volume of gas required to complete a layer of carbon monoxide molecules in direct contact with the solid surface. The curve labeled "total adsorption" was obtained by outgassing the sample at 400°C and then admitting carbon monoxide to the system at −183°C. After this experiment, the surface was outgassed at −78°, or indeed within the range from −78° to 0°, the temperature of outgassing not being very critical within this range. Following the outgassing process, the adsorption of carbon monoxide was again measured and was found to follow the curve labeled "physical adsorption." Emmett and Brunauer interpreted these experiments to indicate that in the initial experiment the carbon monoxide was adsorbed first as a chemisorbed layer with the carbon monoxide molecules held by relatively strong chemical bonds to the underlying atoms, and next as a physically adsorbed layer on top of the chemisorbed

molecules. On outgassing in the temperature range from −78° to 0°C, the physically adsorbed molecules were removed, but the thermal energy was insufficient for the removal of the chemisorbed layer, with the result that the extent of physical adsorption could be determined by a subsequent adsorption measurement. It is obvious then that the difference in heights of the two curves represents chemisorption.

From the isotherms for the promoted iron catalyst given in Fig. 1, it is evident again that the two types of adsorption process can be separated. In this case, the relatively large physical adsorption appears to indicate that the small percentage of promoter material present in the catalyst must cover a relatively large fraction of the surface since it is assumed that carbon monoxide is chemisorbed only on that fraction of the surface in which the iron atoms are exposed, but is physically adsorbed on the whole surface. This deduction comprises an important contribution by Emmett and Brunauer to the understanding of promoter action.

Following the completion of these experiments, Dr. Emmett suggested to me that it would be worth while to apply to this problem the technique developed at Amherst for the calorimetric measurement of heats of adsorption. In Fig. 2 are shown the results of these calorimetric experiments (*4*). Emmett and Brunauer, from an interpretation of their curves, would have predicted that the chemisorption process would be superseded by physical adsorption after 0.24 cc and 0.21 cc of carbon monoxide at −78 and −183° respectively had been adsorbed per gram of catalyst. The initial high values in the differential heats of adsorption, of course, indicate chemisorption at 0°, −78°, and even at −183°, although the heat values are lower at the latter temperature. The sharp drop in the heats of adsorption at −78° to the region of 4 kcals per mole at 0.25 cc per g, the point predicted by Emmett and Brunauer as a result of the analysis of their isotherms, supplies experimental confirmation of the validity of their interpretations. This confirmation from the heat data at −183°, while not so strikingly apparent, is nevertheless seen to be present.

Next we shall turn our attention to another important facet of the work to which Dr. Brunauer has contributed heavily. I refer to the experimental and theoretical development of the theory of multimolecular adsorption, known as the Brunauer, Emmett, and Teller theory (*2*). In the early development of the experimental work, Emmett and Brunauer came to the tentative conclusion that the monolayer is complete at the Point B of the characteristic isotherms obtained in many adsorption systems and illustrated in Fig. 1. This conclusion was found to be consistent with the theoretical considerations developed by Brunauer, Emmett, and Teller. The fundamental equation most generally used is given below:

$$p/v(p_0-p)=\frac{1}{v_mC}+\frac{C-1}{C}\,p/p_0.$$

In this equation, v represents volume of vapor adsorbed, p the equilibrium pressure,

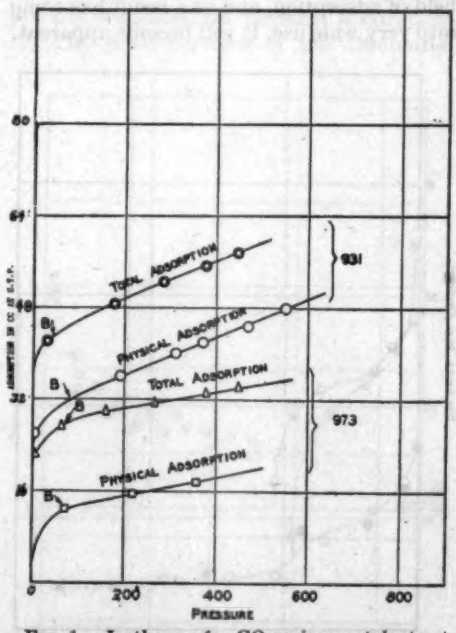

FIG. 1.—Isotherms for CO on iron catalysts at −183°: Pure iron catalyst, 973; promoted iron catalyst (K_2O, 1.59 percent, and Al_2O_3, 1.3 percent), 931.

and p_0 the saturation pressure of the vapor at the temperature of the experiment, v_m the volume of gas necessary to fill the monolayer, and C a constant which is approximately defined as $C = e^{(E_1 - E_L)/RT}$ in which E_1 is the heat of adsorption in the first layer, and E_L is the heat of vaporization, and R and T have their usual meaning, E_1, E_L, and R being expressed in calories and T as the absolute temperature. It is apparent that a straight line should be obtained if the function $(p/v(p_0 - p))$ is plotted against p/p_0, and indeed literally scores if not hundreds of cases provide experimental data which conform to this condition. From such a plot, known as the B.E.T. plot, it is possible to obtain the slope and intercept of the straight line, and from these to calculate the values of v_m and of C. Knowing v_m and making an estimate of the area occupied by the adsorbate molecule on the surface, we can calculate the surface area. Obviously such an estimate of surface area, especially on finely divided or porous solids, is a welcome additional technique in the field of adsorption, and as a result has come into very wide use. It will become apparent, I think, in the experiments to be described below, that our ability to estimate the surface area has added greatly to the significance of the interpretation of the experimental results.

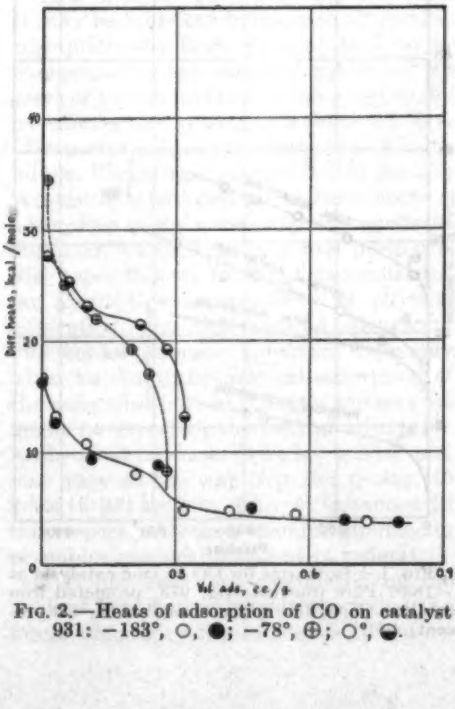

FIG. 2.—Heats of adsorption of CO on catalyst 931: −183°, ○, ●; −78°, ⊕; 0°, ◒.

Now I want to tell you about some work we have been doing in Amherst on the heats of adsorption of vapors on carbon black (*5*). This work was initiated about a year ago as a result of a series of conversations between the writer and Dr. Walter R. Smith of Godfrey L. Cabot, Inc. At that time Dr. Smith was especially interested in getting at the fundamental reasons for the effectiveness of carbon blacks in reinforcing rubber. The investigations in his laboratory had indicated that different carbon blacks presented a very high degree of specificity in their reinforcing ability, especially as determined by the modulus of elasticity. It is apparent, since the carbon black is dispersed through the rubber as particles of the order of several hundred Ångstroms diameter, that the binding forces between the particles of black and the rubber must be adsorption forces between the surface of the blacks and the adjacent sections of the rubber molecules. Following this line of reasoning, one would be tempted to predict that a close correlation might be found between the extent of surface of a carbon black pigment and its reinforcing ability.

That the above prediction is not borne out by the experimental facts is shown in Table 1, which contains data on two samples selected from the several blacks which have been studied; these are labeled Spheron Grade 6 and Graphon respectively. More

TABLE 1

Sample	Diameter	Area	Modulus, 300% elong.
	Å	*sq. m./g.*	*lb. sq. in.*
Spheron Grade 6	285	110	1,720
Graphon	300	85	230

complete data on these blacks especially as regards the chemical composition of their surfaces and their underlying crystal structure have been published elsewhere (*5*). Spheron Grade 6 is a channel black having good rubber reinforcing ability. The Graphon has been prepared from the Spheron

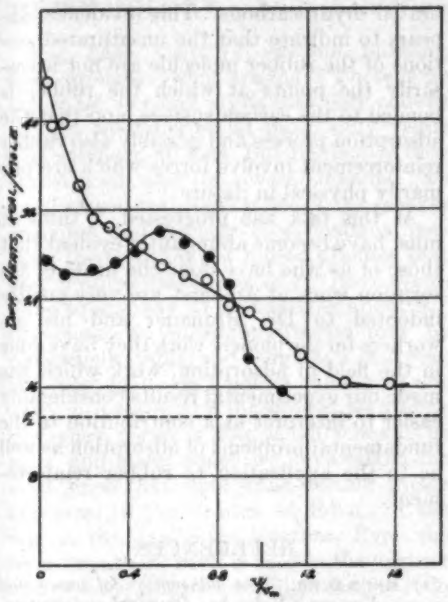

FIG. 3.—Heats of adsorption on carbon blacks at −195°C: N_2 on Spheron Grade 6, ○; N_2 on Graphon ●.

Grade 6 by electrical heating to temperatures of 2,800 to 3,000°C (*6*). As is seen in Table 1, this heating process had relatively little effect on the particle diameters as determined by the electron microscope and on the specific surface areas as determined by the B.E.T. nitrogen adsorption method, although there was a profound effect on the rubber reinforcing ability as indicated by the modulus of rubber samples reinforced by the two pigments.

Although there appeared to be no definite correlation between the extent of surface and the rubber reinforcing ability, it seemed possible that there might be a significant difference in state of surface if the two blacks were compared. Since a convenient means of testing the quality or state of surface of these carbon black samples seemed to be offered by data on heats of adsorption, calorimetric measurements have been undertaken using the elementary gases nitrogen, oxygen, and argon at −195°C (*7*). The results of the experiments with nitrogen are shown in Fig. 3 in which the differential heats of adsorption are plotted against the number of layers adsorbed ($v/v_m = 1$ represents a monolayer).

In relation to the rubber reinforcing problem, the difference in behavior of the Graphon and the Grade 6 black is of great interest. It is apparent that although the high temperature treatment, by which the Graphon was made from the Grade 6 carbon black, resulted in a relatively small change in the total surface of the black, this treatment nevertheless produced a profound change in the energy state of the surface, with the result that approximately the first third of the surface would hold adsorbate molecules considerably less firmly than the original Spheron Grade 6. Thus there appears to be a correlation between the rubber reinforcing abilities of the two blacks and the energy states of their surfaces as indicated by the heats of adsorption.

Apart from the problem of rubber reinforcement several other conclusions of considerable general interest may be drawn from the data of Fig. 3. (1) The order of magnitude of the heats indicates that the process is exclusively physical adsorption. (2) The rapid decrease in the differential

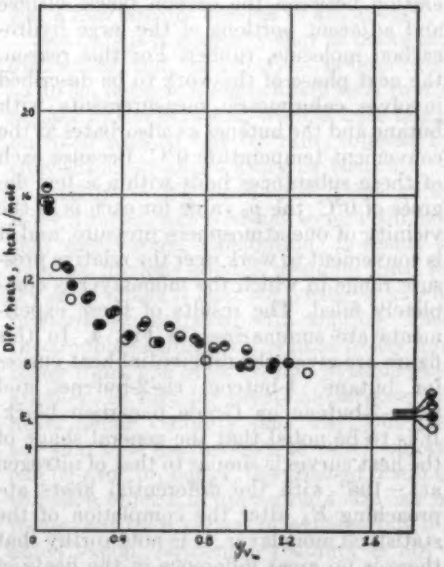

FIG. 4.—Heats of adsorption of Spheron Grade 6 carbon black at 0°C: n-butane, ○; 1-butene, ●; cis-2-butene, ◒; trans-2-butene, ◓.

heats of adsorption for the first parts of the surface covered on the Grade 6 carbon black suggests a considerable heterogeneity of the surface with the fractions first covered exhibiting excessively high binding energies. (3) The rapid approach in the vicinity of $v/v_m = 1$ of the differential heats for nitrogen on all the carbon surfaces studied to values only slightly greater than E_L, the heat of vaporization of nitrogen, provides experimental confirmation of the B.E.T. theory since the value of v_m is determined from the experimental data by a method which is entirely dependent upon this theory.

Calorimetric studies with the elementary gases oxygen and argon produced results entirely analogous to those for nitrogen, the adsorption of both these gases being exclusively of the physical type.

Although the experiments with nitrogen already described have served to bring out differences among the various carbon blacks used, it is obvious that heats of adsorption using hydrocarbons as adsorbates would provide data which would be more closely related to the problem of rubber reinforcement, in which there must be forces in operation between the carbon black surface and adjacent portions of the large hydrocarbon molecule, rubber. For this reason, the next phase of the work to be described involves calorimetric measurements with butane and the butenes as adsorbates at the convenient temperature 0°C. Because each of these substances boils within a few degrees of 0°C, the p_0 value for each is in the vicinity of one atmosphere pressure, and it is convenient to work over the relative pressure range in which the monolayer is completely filled. The results of these experiments are summarized in Fig. 4. In this figure are given the differential heat curves for butane, 1-butene, cis-2-butene, and trans-2-butene on Grade 6 carbon black. It is to be noted that the general shape of the heat curves is similar to that of nitrogen at −195° with the differential heats approaching E_L after the completion of the statistical monolayer. It is noteworthy that there is no great difference in the heats of adsorption of the butane and the unsaturated hydrocarbons. This evidence appears to indicate that the unsaturated sections of the rubber molecule are not necessarily the points at which the rubber is bonded to the carbon surface, and that the adsorption process and possibly also rubber reinforcement involve forces which are primarily physical in nature.

As this talk has progressed, I think it must have become abundantly evident that those of us who have done the heats of adsorption work at Amherst are very greatly indebted to Dr. Brunauer and his coworkers for the pioneer work they have done in the field of adsorption, work which has made our experimental results considerably easier to interpret as a contribution to the fundamental problems of adsorption as well as to the application to rubber reinforcement.

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(6) This high temperature treatment would of course have the effect of removing any volatile matter from the surface of the Grade 6 carbon as well as changing the degree of crystallinity of the underlying solid. Our calorimetric experiments on a sample of Grade 6 black "devolatilized" at 927°C make it more plausible to conclude that the marked difference in the energy states of surface between the Grade 6 black and the Graphon is due to an alteration in the underlying solid by the high temperature treatment rather than to any chemical change in the superficial layers of the pigment (see reference 5).

(7) Details of the experimental method, as well as data on several other blacks and a more extensive discussion are to be found in the publications cited in reference 5.

METEOROLOGY.—*Loose usage of weather words.*[1] W. J. HUMPHREYS.

Fog, haze, visibility, cyclone, blizzard, sleet, drought—what are they? There is no excuse for so using words that they convey no idea whatever or, at most, only vague suggestions of an idea. But frequently they are so used, and for several reasons. The usual reason is the ignorance of the speaker or writer, and the next most common, perhaps, the fact that few indeed practice that best of all courses in rhetoric: "Have something to say; say it; quit talking about it." Of course, an unwilling witness may be consciously vague, and sometimes justly so, but most of us do a lot of talking in our sleep, as it were. And the subject we discuss in vaguest terms is that which concerns us most, and about which we have talked the most since that time when certain things happened in the Garden of Eden "in the cool of the day"—the weather. Even the term "weather" itself is used in the vaguest sort of way, as anyone will realize as soon as he tries to define it accurately.

Not only are we nearly always vague when speaking about weather as a whole, but also, as a rule, we are equally inexact when speaking of any of its elements, or of other phenomena dependent upon or associated with it. For example, *fog* and *haze* are mixed up in a scandalous way despite the great and growing importance of each to the aviator. They often are used as synonymous terms, and again as though a fog were just a dense haze and a haze merely a light fog. Now, they are not the same thing at all and need not often be confused with one another. A true fog is a cloud of water droplets in the space immediately above the surface of the earth—a cloud on the earth. A haze, on the other hand, is a cloud of dust particles of whatever origin, such as impalpable sand caught up by desert winds, the smoke of forest fires, pollen from pine clad mountains, and the like. It may, and commonly does, extend down to the surface of the earth, but it does not always do so. Sometimes, too, its upper surface is as sharply limited and as clearly visible in the direction of the horizon (not vertically) as is that of a waveless ocean.

[1] Received October 10, 1947.

Each decreases visibility. That is why the aviator is so concerned with them. Perhaps, too, as they bother him in much the same way he may feel justified in calling either or both of them by whichever bad name happens to come to his mind first. Indeed they often are so confused, but they do not need to be, for it is quite easy to distinguish between them. The fog droplets are so large that they reflect equally, or nearly so, lights of all colors. It therefore appears white. A large portion of the haze particles, on the other hand, are so small that they scarcely reflect light at all, but just diffuse or scatter it. Furthermore, they scatter the blue, or short-wavelength light, to a far greater extent than they do the longer wavelength, or red light. This being so, the glare that results from the scattered light, and which so greatly reduces visibility, can largely be prevented from reaching the eye by the use of amber, or red, glasses. Clearly, then, the cloudlike obstruction is a dust haze whenever it has a bluish color, and whenever visibility through it is improved by the use of a red or amber screen. Similarly, it almost certainly is a fog when it is white and equally opaque to all colors.

Not only is it generally easy to make this distinction, but frequently it is important to do so, because a fog is likely soon to disappear by evaporation, while a haze hangs on until washed out by rain, thinned by convection, or blown away by clear air.

Visibility.—We have just been glibly using a rather new weather term, visibility, that needs some explanation. It seems to have an obvious enough meaning until we try so to define it that one visibility can be numerically compared with another. Then the trouble begins, for it is hard to say how many times one object is more visible than another. In fact for practical purposes we never define visibility that way, but just say that it is the distance to which objects can be seen, or, better, the greatest distance to which objects of appropriate size can be recognized by a person of average but unaided vision. Evidently visibility is not a distance at all, though dependent on it. Nevertheless, this term with its forced definition is very useful and seems destined

to hold indefinitely an honorable place among the innumerable and growing host of weather words.

It is worth while also to note that there are two quite differently caused visibilities, night visibility and day visibility. The former may be defined as the greatest distance at which a standard light can be seen by a normal, unaided eye. This distance obviously is fixed by the rate of depletion of the light on its way to the observer, since perception fails in this case owing to the feebleness of the light received. Day visibility, on the other hand, is limited mainly by an entirely different cause—not chiefly by depletion of light but by addition of light. When the fog or haze between the observer and distant objects reflects or scatters so much light from the sun, or other sources, as to appear luminous the contrasts of light, by which outlines are perceived and things recognized, soon are lost in the general glare. To repeat, for it is an important distinction, the brighter the light the farther it can be seen. The brighter the day the worse the visibility—the greater the blinding glare.

Fog and cloud.—Not only is fog often confused with haze, to our occasional annoyance, but frequently, too, it is confounded with cloud, and the second of these confusions is the more difficult of the two to disentangle. We may say, and it is quite true, that a fog is a surface cloud formed by surface conditions. But what then shall we call what is left of it when the under portion is burned off, as so commonly happens to sea fog when it drifts in over land? Shall we call it high fog, lifted fog, veillo cloud (the name given to it in southern California), or stratus cloud? All these terms are in good and constant usage, but "stratus cloud" is the best, for that is what the erstwhile fog has now become. Wherever, though, it contacts with the surface, whether water, hill or mountain, it should still be, and generally is, called fog. That is, the same sheet of water droplets is called cloud at one place, where it does not extend to the surface, and fog at another, where it does.

Name of cloud.—And then when we are sure the thing we observe and want to talk about, or record, really is a cloud and not a fog, what sort of a cloud shall we call it, for there are several kinds to which distinct names are given? Way back in the year 1803 an English chemist, or apothecary, as we would say in America, Luke Howard, gave Latin names to several distinct kinds of clouds, names so apt that they soon came into general use in all countries. Later on a few other names were added to the original list until all the more common varieties of clouds had each a distinct designation that seemed entirely appropriate and was clearly defined. But new cloud pictures soon were needed, the old charts being out of print, and this offered an opportunity to revise names and definitions as well. So a committee was appointed for that purpose, a committee that at once began "throwing monkey-wrenches," or so some think, into the erstwhile smoothly running cloud machine. We used to be quite sure, for instance, that a nimbus cloud was one from which rain or snow was falling in quantity, or looked as though precipitation from it was imminent. This was pretty satisfactory, and many of us want to keep the good we have. But no, we are told by this committee that those who feel that they really must retain this life-long name may be graciously permitted to do so provided they pledge to mean thereby only that insignificant little cloud that drifts along in the rain somewhere between the surface of the earth and the base of the heavy cloud out of which the rain is falling. That is, this ancient and honorable name must go out of use altogether, or else be given to that little, unimportant ragamuffin the sailor calls "scud." This is not clarification. It is needless confusion.

Then, too, we felt as certain as one could about anything that the good and appropriate name "cumulo-nimbus" had come to stay and to mean a cumulus (wool-pack or heaped-up) cloud from which rain was falling, and in which thunder and lightning nearly always occur. No. We now are asked to restrict this name to the cumulus cloud whose top has been, or is in the process of being, drawn out into a thin, fibrous sheet. And this in spite of the fact that often a thunderstorm cloud, a cumulo-nimbus as heretofore known, may give much rain and

yet produce no fibrous sheet at all; and in face, of the further fact that occasionally a cumulus cloud may produce no rain, but, for all that, a high sheet of fibrous cloud. For generations we have been accustomed to consider the falling of rain from the base of a cumulus cloud the necessary and sufficient condition for calling it a cumulonimbus. Now we are asked to note whether or not a fibrous cloud is developing out of its top—certainly a radical change of meaning, and a wholly unnecessary one.

It would be well, however, for the aviator to remember that, in middle latitudes, a cumulus cloud that is developing a high fibrous sheet very probably, though not certainly, is also, and at the same time, giving rain below with thunder and lightning; and that the region under a cumulus that has not yet begun to display such a sheet, presumably, but not surely, still is free from rain or other disturbance. To him it is a very real warning flag. Nevertheless, this fact does not in the least justify this change of name, a change that is only confusion confounded—or the other way around.

The next monkey-wrench disarranged the alto-stratus cloud. This term used to mean just what it says—a high layer cloud; that and nothing more. Now we are asked to use this term only when portions of the cloud show some fibrous structure. Here, too, is only confusion, for those who insist on this definition insist, too, that this is the rain-producing cloud. That is, we must call that wide-spread, lightning-free cloud, from which rain is falling abundantly, alto-stratus, and at the same time we must not call it alto-stratus unless we can see in it one or more fibrous patches—thread-bare portions, consisting, we believe, of snow particles. Truly, this puts us in much the same puzzled frame of mind as was Pat when the doctor told him to take one pill three times a day!

The cirro-cumulus cloud is another variety that many of us will insist that we know when we see it, since it is just a field of many little cloudlet balls and ripples (a "mackerel sky" if in orderly rows, a "curdle sky" if the cloudlets are numerous and without order) very high and too tenuous to show evidence of shading, or hide the sun. No, again says our authority, this thing that is a cirro-cumulus cloud must not be called cirro-cumulus unless it has been seen to be formed from a cirrus cloud or cirro-stratus, that is, from a thin or relatively dense cloud, respectively, of fibrous form. Said the yeoman to a yokel: "That is a fine hog of mine over there in the barn lot." "I am not so sure it is a hog," said the yokel. "Why not?" "Oh, I didn't see him grow up from a pig." "Damned fool."

Humidity, a term that comes to mind when we are talking about clouds, is one of the most vaguely used of all weather words. Most of us realize that water is somehow or other involved in its meaning, but just how, when we speak of the air as being humid, is not always clear. Certainly the air is not wet in the sense that our hands become wet when we wash them. Perhaps we can run the trouble down this way: Water can and does exist in the gaseous state as well as in the liquid and solid states. Furthermore, the amount of water that can occupy a given space in the form of a gas, for instance the amount that is in a cubic inch of the "empty" space in a tightly closed bottle containing some water, rapidly increases with increase of temperature. Strange as it may seem, too, this amount is not appreciably affected by the presence of other gases, whether added singly or in whatever combination.

From these facts it is evident that the expression "humidity of the air," or "humidity," for short, can mean any one of at least three different things. To be understood, therefore, one must specify which particular humidity he is talking about. If he just says "humidity" he will be nearly as badly off as the fellow who called for Jones at a Welsh college. "Ah Jones"—most of the windows went up. "Tom Jones"—half of the windows stayed up. "I mean the Tom Jones that has a toothbrush."

If one means, as often is meant, the amount or weight of the water vapor in the air per unit volume he must say "absolute humidity." If, however, as is more often the case, one means the ratio of the amount of water vapor actually present per unit volume to the greatest amount that could

exist in the same volume at the same temperature—the ratio, in brief, of the actual quantity to the saturation quantity—it is necessary to say "relative humidity." Finally, we sometimes mean the weight of the water vapor per unit weight of the moist air, in which case the proper expression is "specific humidity." The term "humidity," as qualified in any one of the above three ways, has a definite and useful meaning; but as commonly used in weather talk it seldom conveys a clear idea to the hearer, and rarely contains a definite concept when it leaves the speaker. Generally, such idea as is associated with this term starts muddled and ends fuddled.

Wind direction.—Here is another source of confusion, one from which force of memory alone can protect us, the naming and charting of wind directions. The weather man's wind vane generally is in the form of an arrow of some sort, and so constructed that the point of the arrow always turns to face the wind. In other words it flies *against* the wind. On his maps and charts, however, he gets all his little wind arrows turned the other way around, for here they fly not against the wind but *with* the wind. What, then, does he mean when he says "north wind," for instance? That is a toss-up until he tells us that he always means a wind from the north. Similarly, by "east wind" he means a wind from the east, by "south wind" a wind from the south, and so on for every point of the compass. It is all simple enough—if you don't forget.

Veering and backing.—Not only the word used to designate the direction of a wind can be confusing, as just explained, but also, and to an even greater extent, those commonly employed to specify the order of its change of direction. Whenever the wind at a particular place so shifts, or changes in direction, as to cause the wind vane to turn clockwise we say that it is veering, and when it so changes as to cause the vane to turn counter clockwise we say it is backing. This, too, is very plain and easy, if only we could remember it—if we could keep from getting like the old lady who said she knew that good eggs sank or swam, but had forgotten which. Indeed it is even worse than that, for while, as used by most writers, backing is backing, and veering is veering, the world over, others reverse the terms with change of hemisphere, calling that shifting of direction veering, in the southern hemisphere, which, in the northern they call backing; and in the southern backing, that which, in the northern, they call veering. Surely the way some words are used is perplexing.

Surface wind is another of the vague terms often used in discussions of the movements of the air. The trouble comes from the uncertainty as to where and what the "surface" is. In the case of calm water there is not much doubt about where the surface is, at least in the every day practical use of the term surface. Over land, however, we usually find a greater and greater entanglement of grass and other vegetation before reaching the soil, and therefore are quite unable to say just where the "surface" is. But in any case where there is a definite surface, as over a lake, for example, the air exactly at the surface we believe to be calm, whatever the wind at appreciable heights above that surface. It would seem then that by the expression "surface wind" we should always mean no wind—no horizontal movement of the air. However, this strictly face value of the expression is not at all what we do mean when we use it. We mean the wind, not at the surface, as the expression implies, but at some appreciable height above it, namely at the level at which the velocity of the air is measured—a level that in practice may be anything from a few feet to twice as many hundred feet above the ground. Clearly then, since the velocity of the wind increases rapidly with increase of height, and since hills and hollows, trees, buildings, and every other irregularity of, and on, the ground affect the velocity of the lower air, the term "surface wind," like the expression "quarter of a lot," has the semblance of meaning something definite but only the semblance.

Fair.—Certainly everyone should know the exact meaning of this term, but many do not and refuse to learn. We may use as vague terms as we like when merely making talk about the weather, but the few terse sentences used by the forecaster certainly should be clearly and correctly understood

by all who read or hear them. Indeed they are clearly understood for they are very carefully constructed to that end, but unfortunately they are by no means always correctly understood. The chief confusion arises from a single one of the forecaster's terms, "fair"—his favorite word (formerly, at least) if one might judge from the frequency of its occurrence. The trouble here comes from the fact that the forecaster and a large portion of his audience, that is, the general public, attach entirely different meanings to this familiar word. He means fair weather; they mean fair skies. To him it is the antonym of foul, and means weather suitable for outdoor occupation; to them it is the antonym of clouds and implies abundant sunshine.

What to do about it is the question. It is precisely the word to use, or certainly would be if rightly understood, but unfortunately there are many who do not know what it means in this connection, and who even are unwilling to learn—who resent being told, who insist that they know what words mean without looking them up in a dictionary or having some smart Aleck tell them. It is too bad, this confusion of meanings, and the worst of it is, there is no obvious and simple way to make the matter any better.

Cyclone.—This is another weather term that often fails to carry the meaning intended. Many people call the "twister" a cyclone, that smallest but most violent, freakish and destructive of all storms. The meteorologist calls this madly whirling devil a tornado, but the man from Missouri, or a neighboring State, comes back with emphasis: "What are you talking about? If my cyclone cellar isn't a place for dodging cyclones, then you will have to show me." This much of the confusion comes mainly from giving the same name, "cyclone," to two entirely different kinds of storms, and two different names, "cyclone" and "tornado," to the same sort of disturbance—the same name to two things, and two names to the same thing.

But this is not all the confusion. If you turn to the word "cyclone" in some large dictionary, or even a meteorological vocabulary prepared by experts, you are likely to find its definition to be: "An area of low atmospheric pressure," or some similar expression. Now, this is inexcusable confusion. A cyclone is not an area at all, any more than a house is an area. It is first and foremost a system of winds, and secondly, characterized by such and such particulars (extensive and about a center of low pressure, accompanied by clouds and widespread precipitation, et cetera) as suffice to distinguish it from all other systems of winds; just as "man" is first of all an animal, and, secondly, possessed of certain qualities, such as rationality, risibility, or whatnot, that belong to no other animal. This is just a fine example of the innumerable cases in which we neither say what we mean nor mean what we say.

Anticyclone.—By its very name one would expect the anticyclone to be something quite the reverse of the cyclone. And so it is in several respects. It occupies a region of relatively high atmospheric pressure, not low, as does the cyclone; its winds, like those of the cyclone, are directed spirally about its center, but outward, not inward, and in the opposite sense; it is attended, usually, by clear skies, not overcast, and fair weather not foul. Meteorologically the cyclone and the anticyclone are distinctly antithetical. Lexicologically, however, they usually have one important feature in common, for generally each is erroneously defined as an area, and not correctly defined as a particular system of winds.

Secondary.—If one were in a teasing frame of mind and wanted some fun with a meteorologist, he hardly could find a better way to succeed than by asking him what a secondary is. He uses this term a lot, and by it he always means a cyclonic storm. Likely as not he will tell you that a secondary cyclone is any one that is not a primary; and that a primary cyclone is any one that is not a secondary. Often, and especially in certain regions, there develops on or near the outer border of a system of cyclonic winds, and doubtless incident to them, a similar system, at first of relatively small extent and strength, but which later grows in size and intensity until, in many cases it itself becomes the main storm, and

occasionally even the only one. This storm, at least in its earlier stages, is often called a secondary cyclone, or secondary, for short, But other storms of less certain history also have been called secondaries for reasons that sometimes seem to be known only to the perpetrator—and he never tells. Yes, ask a meteorologist about secondaries and pretty soon you will have him in a corner, if not up a tree.

Blizzard, a good example of things confused, is a fine word, even if its pedigree is unknown, so long as it means a cold, driving wind filled with blinding snow—a sure-to-goodness storm of the Plains. But how unworthy of itself, how fallen, how decrepit and addled it is when made to mean only a little snow flurry that even a baby scarce would notice. A real blizzard commands respect and deserves a strong name all its own, but the little parlor things that in the Eastern States we so often braggingly call blizzards are not worthy, in comparison, even to be called "blizzetts." A blizzard used to be a blizzard with no doubt about it; now it is anything that has a little snow in it, but one never knows exactly what.

Hail.—Here is a mix-up, too. As used by the U. S. Weather Bureau, and by many people all over the country, only lumps of ice that fall in thunder storms are called hail. Lots of others, though, will not hear of this restriction, but insist that the little ice pellets that fall only in the winter shall also be called hail. That is what they call them, as of course they have a perfect right to do, despite the confusion it causes, and there is no help for it, swear as you may.

Sleet.—Hail is bad enough in its confusion, but sleet is worse, for it has three entirely distinct and much-used meanings. As sung by the poets, and as recorded by the U. S. Weather Bureau, sleet is that which rattles against the windowpane and nothing else. It is frozen raindrops and occurs only in winter and when the temperature of the lower air is below the freezing point while that of the air at around 500 to 1,000 feet elevation is distinctly above it. The rain that falls from the warmer air is frozen by the colder layer below, and reaches the surface in the form of small roundish pellets—ice shot. This is sleet, according to one definition and extensive usage. According to British usage, though, and the custom of many in America also, sleet is a mixture of rain and snow. They call the frozen raindrops hail, or winter hail. This is confusion enough, but by no means the whole of it, for the engineer, ignoring both the above definitions, restricts the name sleet to the smoothish coats of ice that sometimes form on wires, street-car rails and other exposed objects. This sleet, in the engineer's sense of the term, the U. S. Weather Bureau calls glaze. It is the characteristic feature of an ice storm.

And nothing can be done about it. Even if we could get the engineer to call glaze (an excellent word) that which he now calls sleet and restrict this term, as so many of us do, to the stuff that rattles, there still would remain the mixture of snow and rain to rechristen. Of course, a word-telescoping genius like Lewis Carroll might call it "snane," but then some one would be sure to laugh at it, if he did not even make a face.

Ice-flower.—If sleet is a vague term owing to its three definitions, what shall we say about "ice-flower" that means any one of five distinct things? Two or three generations ago Tyndall tried the happy experiment of putting a sheet of ice in the usual position of a lantern slide, and was rejoiced to see blossom on the screen a beautiful bed of six petaled flowers—images of course, of internal melted ice crystals. These 6-rayed cavities Tyndall, in his poetic way, and very appropriately, called ice-flowers. Pretty soon some one else, also with a poetic fancy, gave the same name to the beautiful fernlike figures Jack Frost traces on your windowpane. Then some nature lover, enthusiastically describing the tufts of frost that sometimes in bitterly cold weather grow up numerously on a sheet of ice, called them also "ice-flowers." Even the little columns of ice that spring up from damp soil, and resemble in a measure the familiar Indian pipe, have been called ice-flowers. Finally, at least finally up to the present, the exquisite, curling and satiny ice ribbons, as thin and broad as the blade of a case knife, and often fully as long, that in early winter grow out from the dead stems of rock mint, likewise have been called ice-flowers.

And no wonder, for among the brown leaves of the woods these ice formations look for all the world like a field of beautiful white lilies.

In every one of these five cases the name ice-flowers seems appropriate, yet its use for more than one, preferably the first, is to be deprecated, for it leads only to confusion.

Climate.—It is a long call in one sense from ice-flowers to climate, but the terms have one bad feature, muddle meaning, in common. Perhaps climate is most often defined as "average weather." That would seem to justify defining weather as irregular climate. However, both these snap definitions are hopelessly inadequate. The average annual temperature of a place, average rainfall, and average all-the-other-things do not tell the story of its climate. We need for this purpose to know also the extremes, frequencies of such and such values, normal run of the weather elements through the year, and a lot of other matters as well. In short, the climate of a place is neither its average weather nor the average of its weather, but the history of its past weather.

Drought.—This much used term also is a term confused. Most of us use it glibly as though we knew exactly what it meant until, perchance, we need to define it precisely whereupon our concept of it begins to fade away. We ask ourselves whether it is a number of consecutive days without rain or snow, and, if so, how many. Then we wonder whether the amount of the immediately preceding precipitation should be considered in our definition, and whether the time of the year matters. And if we take a certain number of consecutive days without rain as our definition of drought for one region will that also hold for every other region? Would such a drought for New England be also a drought for New Mexico? If not, then what is a drought? Most of us will agree that one one-hundredth of an inch of rain will not break a drought, and if it will not then certainly we can not define a drought as so many consecutive days without any rain at all, though sometimes, and for statistical purposes, such a rainless spell has been called an absolute drought. Perhaps we might better define a drought as so many consecutive days without more than a specified small amount of rain. But this does not avoid the difficulties as to season and place.

Really, drought is a hard term to deal with. Possibly the best thing to do whenever accuracy is essential is to give it an arbitrary, but reasonably exact, definition appropriate to the needs of the occasion.

Spring.—Who knows when spring begins, or any of the other seasons, for that matter? If we take Tennyson's dictum that it is "when a young man's fancy turns to thoughts of love," we must conclude that it is a perpetual season! If, on the other hand, we take the word at its primitive meaning, signifying the season when plants spring up, we will be forced to conclude that the beginning of spring varies from place to place and year to year, and also that it depends on the kind of plant selected as the criterion. We will even conclude that it has no beginning in ice covered regions, and that it goes on forever in tropical lands.

This indefiniteness led to the more or less general adoption of fixed dates for the beginning and end of each season. In so doing the spice of life was amply preserved for surely in the dates of these beginnings there is abundant variety. According to popular English usage spring begins with the first of February. In America we put it a month later, first of March. Astronomers, though, say it begins with the vernal equinox, which may be at any instant through the day, usually, but not always, on the twenty-first of March, and lasts until the coming summer solstice, also a slightly variable date. Here are three widely used but distinctly different dates for the beginning of spring. February 1, March 1, and March 21 or 22, spread over a period of seven weeks. And so it is with the other seasons, for they follow each other at approximately equal intervals of time. Clearly, then, "first day of spring," "last day of summer," and all others of their kind are confusing expressions for to equally well informed people they convey the concept of distinctly different dates.

End of twilight.—When the end of twilight comes is another point about which the astronomer and the general public have occasion to differ, though confusion in this

case is not nearly so bad as it is in regard to the beginning of spring. The astronomer who, for most of his work, prefers the clearest skies and the darkest hours, says twilight ends only with the last trace of scattered sunlight in the western heavens. On clear nights, the only kind in which he is interested, this last glimmer disappears when the center of the sun is about 18 degrees below the horizon. This does not suit the average person who considers twilight to end as soon as it gets too dark for people to go about their ordinary outdoor occupations. On clear evenings this occurs when the center of the sun is about 6° below the horizon, or in about one-third the time from sundown to the end of astronomical twilight. On cloudy evenings twilight, in this work-a-day sense, ends much sooner, but there is no fixed time for it—the term is vague and often confusing.

Light and dark of the moon.—Whoever follows the foolish occupation of moon-farming, of planting things that fruit above the ground in the light of the moon, and tubers that grow under the soil in the dark of the moon, is confronted with the puzzling necessity of knowing just when it is light of the moon and when dark. And the more he tries to be certain of the matter the more confused he is likely to become. Some will tell him that dark of the moon is that brief time, three or four days, before new moon when it is not seen at all owing to its nearness to the sun, and light of the moon the three or four days centered about full moon. Others, while agreeing with this definition of dark of the moon, will insist that all the rest of the time is light of the moon. Still others, with equal assurance, will insist that the time the moon is waxing, that is, the time of the first and second quarters, is light of the moon, and that the time of its waning, the duration of the third and fourth quarters, is dark of the moon. Finally, there are many who recognize light of the moon to be all the days when the moon is above the horizon most of the fore part of the night, and all the rest of the time dark of the moon.

Here are three distinct and widely recognized definitions of dark of the moon, and four of the light of the moon. What then can the poor moon farmer do when up against such conflicting definitions as these? Nobody knows, though it is quite certain what he should do—forget the moon foolishness and plant when the ground is ready and the season right, as all sensible farmers do.

And these listed above are not the only weather words loosely used by the general public, whose omnibus excuse is the fact that, if restricted to correct understanding and clear expression, the pleasant glibness of its tongue would be lost in many a dreary silence—an honest enough excuse but a mighty poor one.

PALEONTOLOGY.—*An interesting occurrence of fossil tracks in West Virginia.*[1] DAVID H. DUNKLE, U. S. National Museum. (Communicated by C. L. GAZIN.)

Recently, through the generosity of Harold T. Stowell, of Westmoreland Hills, Md., the U. S. National Museum received a small slab of rock exhibiting two distinctly impressed tracks. The following brief report on the specimen is prompted by the probable tetrapod nature of the prints and by the geologic details of its occurrence.

The prints are impressed upon the upper undulating surface of a thin block of dense, finely grained sandstone, composed of many small cross-bedded laminae of variegated red and buff color. As found by Mr. Stowell in 1939, the slab lay loose at the base of a cliff on the East Bank of the Greenbrier River, about 1 mile south of the mouth of Island Lick Run, in the Watoga State Park, Pocahontas County, W. Va.

The physical expression of fossil trackways is dependent on a number of variable conditions. Seldom are the remains of the causative agent found associated with the prints. The structure of the contributing organ, therefore, must be inferred. This latter is very often difficult because the

[1] Published by permission of the Secretary of the Smithsonian Institution. Received December 5, 1947.

completeness of the impression is subject, at the time the tracks were made, to the composition and the consistency of the substratum and, subsequently, to all the multiple, special factors for their preservation. The interpretation of the present examples is not exempted from these general difficulties.

The two tracks, oriented parallel to each other, are each composed of the depressions made by three, stout, distally tapering digits. When the block is placed with the divergent extremities of the impressions directed away from the observer (Fig. 1), it may be seen that the track on the left is more deeply impressed and is situated slightly above the one on the right.

From the similar lengths and practically identical angles of divergence of the corresponding digital impressions in each of the two tracks, it is conceivable that the prints could have been made by one and the same appendage of an animal crossing the photograph transversely. In this latter event, however, the limb would seem necessarily to have extended out at a right angle to the longitudinal axis of the body with little or no anterior flexure. The literature on fossil trackways fails to reveal the occurrence of such a structural condition in the Late Paleozoic. Further in opposition to such an interpretation is the fact that the distance between the two prints would represent an extremely short stride, especially when the slab is large enough transversely to exhibit both preceding and succeeding impressions.

During the tetrapod propulsive cycle (Schaeffer, 1941; Evans, 1946), the body weight, first distributed over the entire surface of either the hand or foot, progressively

FIG. 1.—Photograph of fossil tracks (U.S.N.M. no. 17656) obtained in the Watoga State Park, Pocahontas County, W. Va. Reproduction approx. ×3/5.

shifts to a final concentration on the medial digits. While variable (Colbert and Schaeffer, 1947), this action ideally results in a deeper impression of the inner side of the organ than any other of its parts. The sides of the present fossil footprints, which are adjacent to each other, are clearly more deeply impressed than their distant sides. Thus, while the evidences are conflicting, it is here assumed that these tracks were made by a limb and its complement from the opposite side. Whether that pair of appendages was anterior or posterior can not be ascertained.

Markings of the pads of either heel or palm are not discernible. Impression of the digits alone indicates that the tracks were made on a fairly firm substratum. As pointed out by Colbert and Schaeffer (1947), the lateral digits are structurally the most divergent and under the least optimum of conditions are the most poorly defined in trackways. Thus, while 3-toed impressions have been encountered in practically every known geologic occurrence of tetrapod tracks, it remains uncertain whether complete impressions are being dealt with in the present case. No attempt here is made to assign these prints to any of the scientific names available because of the above mentioned uncertainties of interpretation and because of the questionable advisability of such practice. The specimen is no less interesting for this failure, however, because an early and unknown animal of considerable dimensions is indicated. The better preserved left imprint measures roughly 35 mm across the proximal base of the digits. The distance between parallel lines projected through the medial borders of the prints approaches 62 mm.

The block of sandstone bearing the impressions may be assumed to have been derived at or very near the site of its discovery. No evidences of transportation can be observed. The edges of the slab remain sharply angular. Fragments of soft red shale still adhere in the concavities on both its upper and lower surfaces. Furthermore, its lithology is identical with that of the bedrock exposed in the immediate vicinity as well as for many miles upstream. Price (1929) identifies this rock as the Pocono formation. As summarized by Branson (1910) and Colbert and Schaeffer (1947), the oldest previous reports of definitive tetrapod tracks in the United States are restricted to the upper Mississippian Chester Series. The literature (Butts, 1940; Chadwick, 1935; Willard, 1936) suggests a transgressive character to the Pocono sediments and a consequent variation in age from place to place. Notwithstanding, the Pocono is well below the Chester equivalent, the Mauch Chunk, in the West Virginia section. The present occurrence, therefore, seems to be the earliest yet known from our country and approaches in age the oldest authenticated tetrapod tracks from the Horton Series of Nova Scotia (Dawson, 1894; Sternberg, 1933).

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MYCOLOGY.—*The swarm-cells of Myxomycetes.*[1] EUGENE W. ELLIOTT, State University of Iowa. (Communicated by G. W. MARTIN.)

Biflagellate swarm-cells have been observed as occurring occasionally in Myxomycetes by many observers, but it has been generally held that the swarm-cells in this group are normally uniflagellate. DeBary (*1*) in 1884 and Vouk (*20*) in 1911 reported biflagellate forms in exceptional cases only. Gilbert (*6*) found one-fourth of the swarm-cells of *Stemonitis fusca* biflagellate. Von Stosch (*19*) saw biflagellate cells in many other species, but none in the single species of *Stemonitis* that he studied. Gilbert (*7*) reported *Dictydiaethalium plumbeum* to be uniflagellate, but E. C. Smith (*17, 18*) twice within the following year published photomicrographs showing biflagellate swarm-cells in the same species. Howard (*10*) termed biflagellate swarm-cells in *Physarum polycephalum* "common." Sinoto and Yuasa (*16*) studied *Ceratiomyxa* and four species of Myxogastres, finding only one flagellum in *Ceratiomyxa* but occasional bi- and even triflagellate forms in all the others. Yuasa (*21*) likewise found bi- and triflagellate cells in *Fuligo septica*. Jahn (*11*) in 1928 ignored the previous reports of biflagellate swarm-cells; and in 1936 (*12*) he criticized the work of Von Stosch, insisting that swarm-cells are normally uniflagellate and that all biflagellate swarm-cells are anomalies. Karling (*13*) in his general summary of the literature pertinent to the relationships between the Plasmodiophorales and the Myxomycetes, says that "although the majority are uniflagellate, zoospores with two flagella are not uncommon. . . ." In 1945 Ellison (*5*) reported biflagellate swarm-cells in proportions varying from 2 to 26 percent for a number of species of Myxomycetes but retained the assumption that the majority are uniflagellate.

It is not inconceivable that an occasional biflagellate swarm-cell could occur as an abnormality in an otherwise uniflagellate group. But biflagellation has already been reported too frequently in the Myxomycetes to represent mere abnormality. And on the basis of flagellation as reported in other groups it is very unlikely that both uniflagellate and biflagellate forms would normally exist in the same life stage of the Myxomycetes. It seems more probable that the second, shorter flagellum is difficult to see and is frequently hidden. This study was undertaken to determine whether this latter assumption is not the case.

[1] Received December 15, 1947.

METHODS AND MATERIALS

Spores of 21 collections representing 11 species were germinated for these tests. Following is a list of the collections, with the State and year of collection:

1.	*Arcyria denudata*	Iowa	1947
2.	*Dictydiaethalium plumbeum*	Iowa	1947
3.	*Dictydiaethalium plumbeum*	Iowa	1947
4.	*Enteridium rozeanum*	Iowa	1946
5.	*Enteridium rozeanum*	Iowa	1946
6.	*Enteridium rozeanum*	Iowa	1947
7.	*Fuligo septica*	Indiana	1944
8.	*Fuligo septica*	Michigan	1947
9.	*Lycogala epidendrum*	Indiana	1944
10.	*Lycogala epidendrum*	Iowa	1946
11.	*Lycogala epidendrum*	Iowa	1947
12.	*Oligonema schweinitzii*	Iowa	1947
13.	*Oligonema schweinitzii*	Iowa	1947
14.	*Physarum polycephalum*	Iowa	1947
15.	*Reticularia lycoperdon*	Iowa	1929
16.	*Reticularia lycoperdon*	Iowa	1942
17.	*Reticularia lycoperdon*	New York	1947
18.	*Reticularia lycoperdon*	Iowa	1947
19.	*Stemonitis flavogenita*	West Virginia	1947
20.	*Stemonitis splendens*	Iowa	1947
21.	*Trichia affinis*	Iowa	1946

The first cultures were prepared in September 1946, using *Reticularia lycoperdon* (Coll. no. 15, above), *Lycogala epidendrum* (no. 10), and *Trichia affinis* (no. 21). Abundant germination was obtained in the first attempts with *R. lycoperdon* and *T. affinis*, but only one of several cultures of *L. epidendrum* was observed to germinate. Repeated attempts to germinate cultures of the two oldest collections of *Enteridium rozeanum* (nos. 4 and 5) resulted in the observation of occasional swarm-cells in no. 4, but only very low percentages of germination. Other species gave similar results.

Cultures were made in Syracuse watch glasses using distilled water from which the traces of toxic minerals were removed with powdered charcoal. It was observed fre-

quently that most of the spores placed in the culture dish continued to float on the surface of the water, never becoming wet. This was especially true of those forms having very small spores, such as *Enteridium* and *Lycogala*. Various wetting agents were tried in an attempt to accelerate the wetting and increase the germination of these spores.

Alcohol was the first wetting agent tested. Cayley(*4*) used 20 percent alcohol for wetting spores of *Didymium* sp., securing approximately 50 percent germination whether the spores were in the alcohol "a few minutes" or a full hour. Also she used a solution of 0.2 percent mercuric chloride in a mixture of equal parts of 95 percent alcohol and water, as a combined wetting agent and bactericide. The resulting cultures were not bacteria-free and germination was poor.

In the tests here reported a number of different dilutions of alcohol were tried, 95 percent being the strongest and 20 percent the weakest concentration used. Lower concentrations had negligible wetting effect. After wetting, spores were washed three times with centrifuging and cultured as before. No germination was obtained from spores wetted with alcohol, even in *Reticularia lycoperdon*, in which nearly 100 percent germination had been secured without the use of a wetting agent.

Trisodium phosphate was tried next. By experimentation it was found that spores of *Enteridium rozeanum*, which has the smallest spores of any species used up to the time of these tests, would sink immediately in a 0.5 percent solution and slowly in a 0.2 percent solution. In preparing cultures using trisodium phosphate as a detergent, washing was done as when alcohol was the wetting agent. Cultures of *E. rozeanum* (no. 5), prepared with the use of trisodium phosphate in either 0.5 percent or 0.2 percent solution, germinated nearly 100 percent within one hour. Swarm-cells had not been seen in cultures of this collection before.

Trisodium phosphate in 0.5 percent solution was used as a detergent in preparation of cultures of a number of other collections. Abundant germination was produced occasionally in *Lycogala epidendrum* (no. 10), but no consistent germination was secured with any species except *E. rozeanum*, hence toxic effects were suspected.

In the search for an active detergent without toxic effects, two commercial detergents, "Soilax" and "Dreft," were tried, both in 0.5 percent solution. Both acted as effective wetting agents, but many spores were caught in the foam on the "Dreft" solution and could not be reclaimed by centrifuging. Germination in cultures thus prepared was similar to, and no more satisfactory than, that obtained with trisodium phosphate.

The fact that bile salts greatly lower the surface tension of solutions, as exemplified in the Hay test for bile in urine (*9*), inspired the testing of solutions of sodium glycocholate and sodium taurocholate as wetting agents for preparation of cultures. It was found that either the glycocholate or the taurocholate will wet the spores satisfactorily in 1 percent solution, but lower concentrations act so slowly as to be inadequate. In equal concentrations, these two salts, or a mixture of them, are equally effective as wetting agents.

Spores of many species of Myxomycetes that did not germinate at all when prepared directly in water or with other detergents, germinated when previously wetted with either of the bile salts. And all those collections which germinated directly in water germinated more quickly and in greater percentage when previously wetted with the bile salts. Of course, the spores of two different collections of the same species frequently show greatly different percentages of germination. Similar differences are apparent when the bile salts are used, other conditions being equal; but the differences are substantially reduced.

Germination of spores directly in 1 percent solution of the bile salts was tried. Protoplasts emerged from the spore cases, but developed no further. However, a technique was developed in which only one washing with water is necessary to free wetted spores of the detergent. Approximately 1 cc of the wetting agent is placed in a centrifuge tube and the spores to be cultured are added and stirred until wetted, a process which usually takes one-half minute. Then the solution is diluted to 5 or 6 cc with water and promptly centrifuged. The spores are then washed once with distilled water by centrifuging and are finally cultured in

distilled water prepared with charcoal as described above.

It was found that in order to preserve the flagella of swarm-cells for observation, the killing agent used in preparation of material for microscopic examination must act very quickly. Smears prepared by air-drying, or even by drying as quickly as possible over mild heat, as is done in the preparation of bacterial mounts, showed recognizable swarm-cells, all of which, however, completely lacked flagella.

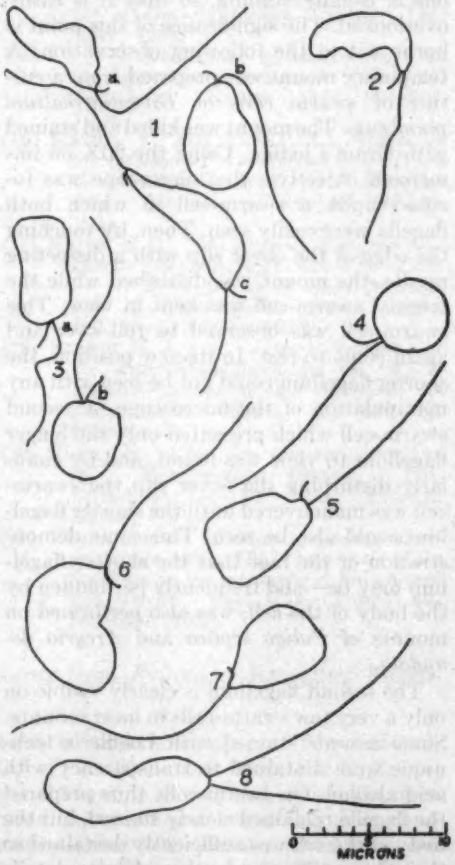
The most satisfactory results for temporary mounts were obtained by killing and staining on the slide with a drop of iodine-potassium iodide solution. For this purpose Gram's iodine is used without dilution, density of staining being controlled by varying the proportions of culture solution and iodine solution which are mixed on the slide. Whatever proportions are used, mixing must be accomplished quickly and thoroughly if the flagella are to be preserved for observation.

Permanent slides were prepared for observation of flagella by use of a modification of the Loeffler stain for bacterial flagella (*3*). The mordant and stain were prepared as directed, but the times of application of both mordant and stain were reduced from five minutes to one-half minute. Smears were prepared by various means. Slides were thinly coated with albumin fixative, on which a drop of culture solution was placed and inverted over osmic acid fumes. This was then allowed to dry in air. Other smears, killed over osmic acid, were fixed by heating gently after drying. Still others were killed with iodine, as was done in the preparation of temporary mounts, and allowed to stand until the iodine had sublimed. These slides were fixed over heat. The crystals of potassium iodide were dissolved off in distilled water before staining. Equally satisfactory results were obtained with all these methods. The iodine method, being the simplest, was used.

RESULTS AND DISCUSSION

Some biflagellate swarm-cells were observed in every culture prepared of every species studied. In some species, notably *Lycogala epidendrum*, *Oligonema schweinitzii*, and *Fuligo septica*, the proportion of swarm-cells obviously biflagellate was nearly 100 percent. In one culture of *Dictydiaethalium plumbeum*, many zygotes with four flagella, in two pairs of two each, were found.

In all species except *Stemonitis splendens*, the two flagella were of greatly different length, the shorter one being sometimes scarcely more than 1μ long. The length of the shorter flagellum is quite constant for a given species. The flagella of *Stemonitis splendens* averaged 16μ for the longer flagel-



FIGS. 1–8.—Outlines of representative swarm-cells: 1, *Dictydiaethalium plumbeum* (*a–b*, swarm-cells; *c*, zygote) (all others are swarm-cells); 2, *Enteridium roseanum;* 3, *a–b*, *Fuligo septica;* 4, *Lycogala epidendrum;* 5, *Oligonema schweinitzii;* 6, *Physarum polycephalum;* 7, *Reticularia lycoperdon;* 8, *Stemonitis splendens.*

lum and 14μ for the shorter. Ellison drew swarm-cells of *Stemonitis fusca* and *S. ferruginea* with two flagella approximately equal in length, though the other biflagellate forms he records have one flagellum much shorter than the other. Similarly, Gilbert (*6*) shows *S. fusca* with two nearly equal flagella, but all other forms definitely heterocont.

The second flagellum, in addition to being very short, is usually recurved so as to be almost indistinguishable from the outline of the cell itself. Even in *Stemonitis splendens*, in which both flagella are relatively long, one is usually trailing, so that it is easily overlooked. The significance of this point is borne out in the following observation: A temporary mount was prepared from a culture of swarm cells of *Dictydiaethalium plumbeum*. The mount was killed and stained with Gram's iodine. Using the 90X oil immersion objective, the microscope was focused upon a swarm-cell in which both flagella were easily seen. Then, by touching the edge of the cover slip with a dissecting needle, the mount was disturbed while the original swarm-cell was kept in view. This swarm-cell was observed to roll over and again come to rest. In its new position, the shorter flagellum could not be seen with any manipulation of the microscope. A second swarm-cell which presented only the longer flagellum to view was found, and by simularly disturbing the cover slip the swarm-cell was maneuvered until the shorter flagellum could also be seen. This same demonstration of the fact that the shorter flagellum may be—and frequently is—hidden by the body of the cell, was also performed on mounts of *Fuligo septica* and *Arcyria denudata*.

The second flagellum is clearly visible on only a very few swarm-cells in most mounts. Some mounts stained with Loeffler's technique were destained to transparency with acid alcohol. On swarm-cells thus prepared the flagella remained clearly stained, but the body of the cell was sufficiently destained so that the nucleus and other cellular details were visible. When swarm-cells are properly stained by this method, careful focusing on the anterior portion of the body of the cell will reveal a dark line reaching backward from the apex to the base of the conical anterior portion. Occasionally this dark line will be found lying across the conical portion, either on or underneath the body of the cell, but most frequently it is barely distinguishable from the outline of the cell. It is not found on those swarm-cells on which the second flagellum is clearly visible apart from the body of the cell. It is believed that this is the shorter flagellum which ordinarily is closely appressed to the anterior portion of the cell.

CONCLUSIONS

Spores of 21 collections representing 11 species of Myxomycetes were germinated. The flagellation of the swarm-cells of these species was observed in temporary and permanent preparations. In three species, *Fuligo septica*, *Lycogala epidendrum*, and *Oligonema schweinitzii*, it was possible to see two flagella on nearly every swarm-cell observed. In all other species, two flagella were clearly visible on some swarm-cells, and it was demonstrated by manipulation of fluid mounts that the second flagellum could be brought into view even though not originally visible. From this it is inferred that if adequate technique were used, the second flagellum would be found on all myxomycete swarm-cells.

All of the biflagellate swarm-cells observed in this study were heterocont. In all species except *Stemonitis splendens* the two flagella differ greatly in length. In *S. splendens* the difference is slight, but is nevertheless constant.

The Myxomycetes and the Plasmodiophorales have long been considered related groups by may investigators. Formerly, the reported existence of anteriorly uniflagellate zoospores in the reproductive cycle of both groups was regarded as strong evidence of this relationship. However, Ledingham (*14*, *15*) showed that the zoospores of the Plasmodiophorales are anteriorly biflagellate, the second flagellum being very short and, hence, easily obscured. As a result of Ledingham's finding, the supposed difference in flagellation was thought by many to emphasize a separation between the two groups.

The existence of two blepharoplasts has been reported in several species of Myxomy-

cetes (*5*, *8*, *19*). Bessey (*2*) regards the second blepharoplast as a vestige of the biflagellate condition, indicating that the Myxomycetes and the Plasmodiophorales have arisen from a common ancestor. He considers the loss of the second flagellum as evidence that the Myxomycetes are of higher phylogenetic position.

Insofar as flagellation is of phylogenetic significance, the existence of the second flagellum in the swarm-cells of Myxomycetes as demonstrated by this study, may indicate a closer relationship with the Plasmodiophorales than has recently been supposed.

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ORNITHOLOGY.—*A small collection of birds from Eritrea.*[1] HERBERT FRIEDMANN, U. S. National Museum.

During the early stages of World War II when North Africa was an important battlefield, numbers of American troops were stationed in Eritrea, a rather neglected and little-known part of eastern Africa. Two of the men who were destined to spend some time in that former Italian colony collected birds as time and opportunity permitted. Col. L. R. Wolfe sent in to the U. S. National Museum a small box containing eight birds. A second and larger shipment comprising about 300 specimens was most unfortunately lost in transit. Thane Riney similarly suffered the loss of the bulk of his collection, but was able to bring back some 37 birds, which he forwarded to the Museum of Vertebrate Zoology of the Univer-

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sity of California, where most of them are now. A small number, chiefly duplicates, were generously presented by that institution to the National Museum, where they together with Wolfe's handful of specimens, are incorporated with the large East African material previously brought together by Mearns and others.

Because of the paucity of published data concerning Eritrean birds I thought it advisable to put on record the contents of the Wolfe and Riney collections, small though they be, and, thanks to the cooperation of Dr. Alden H. Miller and Dr. Frank A. Pitelka, I have been able to examine all the specimens and to combine them in this report. A collection containing only 34 species could hardly be expected to yield many new facts, but as may be seen from the subjoined annotated list, a few items of interest have been found to be contained in it.

Family ACCIPITRIDAE: Hawks, Eagles, and Kites

Elanus coeruleus coeruleus (Desfontains)

Falco coeruleus Desfontains, Hist. (i.e., Mém.) Acad. Roy. Paris, for 1787: 503. 1789 (near Algiers).

One specimen, in somewhat abraded plumage, was collected at Ghinda, altitude 962 meters, on February 2, 1943, by Thane Riney.

Melierax metabates metabates Heuglin

Melierax metabates Heuglin, Ibis 1861: 78 (White Nile between 6° and 7° lat. N.).

During August (16–26), 1942, Wolfe collected an adult male, adult female, and a juvenal male 15–20 miles south of Gura. Riney obtained an adult (unsexed) between Cheren and Agordat, on December 12, 1942. The very extensive white freckling on the secondaries and inner primaries of the adults suggests that they may be somewhat intermediate between *neumanni* and true *metabates*, but nearer to the latter. They have the barred upper tail coverts of the nominate race.

Sclater and Mackworth Praed (Ibis 1919: 702) consider all Sudanese birds south of Khartoum and north of Lake No as intermediate between the two races. Moltoni and Rusconi (Gli Uccelli dell' Africa Orientale Italiana 2: 240. 1942) record *neumanni* from Eritrea near the border of the Red Sea Province of the Sudan, and *metabates* from the rest of Eritrea.

The birds are rather small, the adult male having a wing length of 300, the female of 307 mm. In this respect they approach the southwest Arabian race *ignoscens*.

The August birds show evidences of molting.

The juvenal female is slightly more rufescent on the breast and darker on the upperparts than comparable birds from extreme northwestern Uganda.

Buteo rofofuscus augur (Rüppell)

Falco (*Buteo*) *augur* Rüppell, Neue Wirbelth., Vög.: 38, pl. 16. 1836 (Abyssinia).

A female was taken by Riney about 20 km from Decamera, on January 1, 1943, at an elevation of 2,000 meters. A second specimen is unfortunately without data of any kind. Both are in the light phase.

Lophaetus occipitalis (Daudin)

Falco occipitalis Daudin, Traité 2: 40. 1800 (the Anteniquoi country, i.e., Knysna district, Cape Province).

Wolfe collected a male and an unsexed bird, both adults, 15–20 miles south of Gura, August 16, 1942. He found the crested eagle not uncommon at elevations of about 5,000 feet.

Aquila rapax raptor Brehm

Aquila raptor Brehm, Naumannia 1855: 13 (Blue and White Nile).

An adult female in worn plumage was taken by Riney on a nest in a baobab tree, south of Barentu, January 17, 1943. The date is in agreement with Blanford's observation (Geol. and Zool. Abyss.: 295–296. 1870) that in Ethiopia the birds breed in January.

Circus pygargus (Linnaeus)

Falco pygargus Linnaeus, Syst. Nat., ed. 10, 1: 89. 1758 (Europe).

Riney collected an unsexed bird (female by plumage) 5 km west of Asmara at an elevation of 2,409 meters, on January 4, 1943, when he saw it foraging over open fields. The bird is in rather poor plumage and is molting its remiges, making definite identification somewhat difficult. The third primary, from the outside, is only partly grown in, while the remainder of the remiges is considerably abraded.

Montagu's harrier is a Palearctic bird and

comes into Africa only during the northern winter. In eastern Africa it has been recorded all the way to South Africa.

Family FALCONIDAE: Falcons

Falco naumanni pekinensis Swinhoe

Falco cenchris var. *pekinensis* Swinhoe, Proc. Zool. Soc. London 1870: 442 (Shihshanling (Ming Tombs) near Peking).

An adult male, taken near Asmara, 2,371 meters, on January 31, 1943, by Thane Riney, is in somewhat worn plumage and differs from the nominate race in its darker rufescent back. Archer and Godman (Birds of British Somaliland and the Gulf of Aden 1: 180–183. 1937.) consider this race a rare straggler to their area and suggest that it may follow down the Nile Valley to the west. If we were to apply this suggestion to Eritrea we should have to consider the bird a rare visitor to that country as well, which actually seems to be the case; in fact, Riney's specimen is the first record for Eritrea, at least as far as published data indicate. The bird is known to reach South Africa during the northern winter and is probably commoner along the eastern part of the continent than the few records would indicate.

Falco alopex (Heuglin)

Tinnunculus alopex Heuglin, Ibis 1861: 69, pl. 3 (Gallabat, Egyptian Sudan).

A male, collected by Wolfe, 15 miles south of Gura, August 16, 1942, is noticeably darker than a female from Talodi, Kordofan, Anglo-Egyptian Sudan, the only other example of the species available for comparison. Our male also differs from this female in having the black bars on the median rectrices more complete, less marginal in character, and in having those on the lateral rectrices less broadened. There is a tendency in the female to have these bars dilated marginally on all the tail feathers, but this is most highly developed on the outer ones.

Bannerman (Birds Tropical West Africa 1: 216–219. 1930) has given more extensive comments on this species than any other recent writer. He states that the wing measurement of the males varies from 266 to 293 (our example measures 276 mm). His account is unfortunately garbled by some misprinting of his original intention as he then goes on to state that the "largest birds are those from N. Nigeria with wings ♂ 212 ♀ 210. . . ."

The intensity (darkness or paleness) of the coloration appears to vary individually in this kestrel. Bannerman had 23 specimens for study and found the darkest birds came from such widely separated areas as Kulikoro on the Niger River, Jebel Marra in Darfur Province, Anglo-Egyptian Sudan, and Ethiopia. On the basis of this spotty occurrence of dark birds (among which the present Eritrean example appears to belong) it seems impracticable to recognize Oberholser's race *eremica* from Togoland, which is based entirely on its paler tone. Bannerman (*loc. cit.*) has placed *eremica* in the synonymy of *alopex*, but, probably by oversight, he uses a trinomial for the latter.

Our specimen shows signs of molt in the remiges and rectrices. This suggests the possibility that its darker tone may be due to the freshness of its plumage and that paler birds may show the results of fading under the hot sun in the dry open country it inhabits.

Falco tinnunculus tinnunculus Linnaeus

Falco tinnunculus Linnaeus, Syst. Nat., ed. 10, 1: 90. 1758, (Europe; restricted type locality, Sweden *apud* Hartert).

Riney shot a female 10 km from Decamera, at 2,050 meters, on January 1, 1943. The bird has a wing length of 258 mm and is therefore too large to be *F. t. archeri* Hartert and Neumann from the Waghar Mountains, and also too large to be *F. t. carlo* (Hartert and Neumann) of the mountains of East Africa. In coloration it is slightly darker than typical *tinnunculus* but not as dark as *carlo*. It seems best treated as a darkish example of the nominate race.

Family PHASIANIDAE: Pheasants, Francolins, and Quails

Francolinus erckelii erckelii (Rüppell)

Perdix erckelii Rüppell, Neue Wirbelth. Vög.: 12, pl. 6. 1835 (Taranta Mountains, Abyssinia).

Erckel's francolin is a poorly known bird, and any additional material of it is still of interest. Riney collected a female, 40 km north of Asmara, 2,390 meters altitude, on January 9, 1943. It is smaller than any mentioned by Moltoni and Rusconi (Gli Uccelli dell' Africa Orientale Italiana 3: 22–25. 1944.) having a wing length of only 202.6 (their series ranges from 205 to 230), and a tail length of 91 (as against 110–140 mm in Moltoni's series).

Moltoni and Rusconi write that the race *pentoni* ranges from the Red Sea Province of the Sudan into the adjacent parts of Eritrea, but as far as I can learn no actual specimens of this paler, grayer race have been taken in Eritrea.

Family COLUMBIDAE: Pigeons and Doves

Oena capensis capensis (Linnaeus)

Columba capensis Linnaeus, Syst. Nat., ed. 12, 1: 286. 1766. (Cape of Good Hope).

Riney collected a male in low bush near the Decamera turn off on the Adi-Ugri Road, 45 km south-southwest of Asmara, on January 1, 1943. The bird is in abraded plumage.

Family APODIDAE: Swifts

Apus aequatorialis aequatorialis (Müller)

Cypselus aequatorialis Müller, Naumannia 1: 27. 1851 (Abyssinia).

A female mottled swift was taken by Riney on the Citao compound at Asmara on February 2, 1943. It is a bird in rather worn feathering.

Family COLIIDAE: Colies, or Mousebirds

Colius striatus leucotis Rüppell

Colius leucotis Rüppell, Mus. Senck. 3: 42, pl. 2. 1839 (Temben Province, Abyssinia).

Two unsexed examples of this common species were obtained by Riney, one on the Adi-Ugri Road at the base of the Asmara Plateau, on November 14, 1942, and one near Cheren, on March 15, 1943. This race inhabits Eritrea, Bogosland, northern Ethiopia, and adjacent portions of the Anglo-Egyptian Sudan.

Family CORACIIDAE: Rollers

Coracias naevius naevius Daudin

Coracias naevia Daudin, Traité 2: 258. 1800 (Senegal).

Wolfe obtained one specimen, unsexed, south of Gura, on July 26, 1942. It has the white stripes on the anterior underparts unusually broad and has the purplish brown of the top of the head and the greenish of the back slightly darker than in Ethiopian specimens. It has an unusually large bill, the culmen measuring 47 mm from the base; the largest billed birds from Ethiopia and Kenya Colony seen having culmen lengths of 43 mm or less. The specimen was molting when collected, the outer remiges still showing their sheaths basally.

Family UPUPIDAE: Hoopoes

Upupa epops somaliensis Salvin

Upupa somaliensis Salvin, Cat. Birds Brit. Mus. 16: 13. 1902 (Somaliland).

On February 2, 1943, Riney obtained an adult female on the acacia plain between Nefasit and Decamara, altitude 1,825 meters. It matches Ethiopian and East Africa examples very well. This is the resident race, the nominate one being only a winter visitor from Europe.

Family BUCEROTIDAE: Hornbills

Tockus nasutus nasutus (Linnaeus)

Buceros nasutus Linnaeus, Syst. Nat., ed. 12, 1: 154. 1766 (Senegal).

The gray hornbill is represented by a head collected south of Gura, in August 1942, by Colonel Wolfe.

Tockus erythrorhynchus erythrorhynchus (Temminck)

Buceros erythrorhynchus Temminck, Pl. Col., livr. 36: sp. 19. 1823 (Senegal).

Riney collected a male red-billed hornbill on the plains west of Agadat, 700 meters altitude, on December 13, 1942, and Wolfe shot another individual south of Gura, July 26, 1942. Riney's bird shows active molt in the tail.

Family TIMALIIDAE: Babblers

Turdoides leucopygia leucopygia (Rüppell)

Ixos leucopygius Rüppell, Neue Wirbelth., Vög.: 82, pl. 30, fig. 1. 1840 (coast of Abyssinia).

Riney met with this northern race of the white-rumped babbler 40 km north of Asmara, 2,390 meters altitude, on January 9, 1943, when he collected a male and an unsexed specimen. This race has the whole forehead and forecrown white, while the four more southern subspecies have the white reduced or absent.

The male shows signs of active molt in the wings.

Family TURDIDAE: Thrushes, Chats, and Wheatears

Monticola solitaria solitaria (Linnaeus)

Turdus solitarius Linnaeus, Syst. Nat., ed. 10: 1: 170. 1758 (Italy, *apud* Hartert).

The blue rock-thrush of central and southern Europe winters in northeastern Africa, along

with the race *longirostris* of western Asia. Riney collected a female 20 km south-southwest of Asmara, 1,930 meters altitude, on January 1, 1943, which agrees with the nominate form in its dark and brownish color (the western Asiatic form is paler and more grayish).

Oenanthe hispanica melanoleuca (Güldenstadt)

Muscicapa melanoleuca Güldenstadt, Nov. Com. Petrop. 19: 468. 1775 (Georgia; Caucasus).

An unsexed specimen of the eastern black-eared wheatear was obtained by Riney 20 km south-southwest of Asmara, 1,930 meters altitude, on January 1, 1943. The race is known to winter from Egypt to Darfur Province in the Angol-Egyptian Sudan and to Eritrea and to southwestern Arabia (Aden Protectorate).

Oenanthe lugubris (Rüppell)

Saxicola lugubris Rüppell, Neue Wirbelth., Vög.: 77, pl. 28, fig. 1. 1837 (Simen, Abyssinia).

The Abyssinian black chat was met with by Riney on January 1, 1943, when he collected an adult male and female 20 km south-southwest of Asmara, at an altitude of 1,930 meters. These two specimens are definite evidence that Zedlitz (Journ. für Orn. 1911: 85) was wrong when he suggested that this species was migratory in Eritrea, leaving for the south after breeding, and returning in the latter part of March. Together with December and January birds previously recorded from Ethiopia (Friedmann, U. S. Nat. Mus. Bull. 153, pt. 2: 135. 1937.) these examples indicate that the migration, if any, is very limited in geographical extent.

Both specimens are in fairly fresh plumage; the female has pale tips on the outer rectrices (curiously enough, only on the left side of the tail, but not on the right), the male has none at all.

Oenanthe isabellina (Temminck)

Saxicola isabellina Temminck, Pl. Col., livr. 79: pl. 472, fig. 1. 1829 (Nubia).

Riney obtained a female isabelline chat in the low brushland of the Asmara Plateau, 2,040 meters elevation, near Asmara, on January 1, 1943. The species breeds in Europe and east to Mongolia and winters in northeastern Africa, Arabia, and India.

Cossypha semirufa semirufa (Rüppell)

Petrocincla semirufa Rüppell, Neue Wirbelth., Vög.: 81. 1840 (Abyssinia).

In the dense forest of Monte Marara, 2,340 meters altitude, about 40 km north of Asmara, on January 9, 1943, Riney shot an example (unsexed) of this robin-chat. This must be about as far north as it is known to occur; I know of no published records north of Bogosland.

Family MUSCICAPIDAE: Old World Flycatchers

Bradornis pallidus bowdleri Collin and Hartert

Bradornis pallidus bowdleri Collin and Hartert, Nov. Zool. 34: 52. 1927 (new name for *B. p. sharpei* Rothschild, 1913, not *B. sharpei* Bocage, 1894: Abyssinia).

One female was collected by Riney near Decamera, 2,000 meters altitude, on January 1, 1943. This race of the pale flycatcher inhabits northern Ethiopia (south to the vicinity of Adis Ababa) and Eritrea, where it lives on open bushy areas.

Batis minor erlangeri Neumann

Batis minor erlangeri Neumann, Journ. für Orn. 1907: 352 (Gara Mulata, near Harrar, Ethiopia).

Riney obtained an unsexed specimen (male by plumage characters) near Decamera, 2,000 meters altitude, on January 1, 1943. Inasmuch as this example agrees with Ethiopian specimens of *erlangeri* and shows no approach to the characters ascribed to *chadensis*, I have no hesitancy in so classifying it, although it extends the known range of *erlangeri* northward a very considerable distance. It was known previously from the Harrar area in central eastern Ethiopia, southwest to southern Shoa, and to Lake Stefanie.

Family MOTACILLIDAE: Wagtails and Pipits

Anthus richardi cinnamomeus Rüppell

Anthus cinnamomeus Rüppell, Neue Wirbelth., Vög., 103. 1840 (Simien Province, Abyssinia).

On January 1, 1943, Riney collected a female of this pipit about 15 km south-southwest of Asmara, at 2,040 meters altitude. The bird was seen on the ground in low brushlands. The specimen, which is in somewhat frayed plumage agrees well with others from Ethiopia.

Family LANIIDAE: Shrikes

Lanius collaris humeralis Stanley

Lanius humeralis Stanley, *in* Salt, Travels in Abyssinia . . . , Appendix, li, no. 4. 1814 (Chelicut, Abyssinia).

One female, collected by Riney 15 km south southwest of Asmara, 2,040 meters, January 1, 1943, is of this subspecies, which occurs from Eritrea and Ethiopia, south through eastern Africa (west to central Uganda), to Zululand and Natal.

Family PRIONOPIDAE: Wood-shrikes

Prionops cristata cristata Rüppell

Prionops (*Lanius*) *cristatus* Rüppell, N. Wirbelth., Vög., lief. 183: 30, pl. 12, fig. 2. 1837 (coast at Massawa).

Riney obtained an unsexed bird at Ghinda, 962 meters altitude, on February 2, 1943. The specimen is in very worn feathering.

Family STURNIDAE: Starlings

Lamprocolius chalybeus chalybeus (Hemprich and Ehrenberg)

Lamprotornis chalybeus Hemprich and Ehrenberg, Symbolae physicae, folio y: pl. 10. 1828 (Ambukol, Dongola).

A female, showing evidence of active molting in the wings, was collected by Riney 30 km from Asmara, 1,930 meters altitude, on January 1, 1943.

Family PLOCEIDAE: Weaverbirds

Sporopipes frontalis abyssinicus Mearns

Sporopipes frontalis abyssinicus Mearns, Smithsonian Misc. Coll. 56(14): 7. 1910 (Abyssinia).

Three examples of the speckle-fronted weaver were collected by Riney, 2 males and 1 female, on the acacia plain between Nefasit and Decamera, elevation 1,852 meters, February 7, 1943. One of the males shows evidence of molting in the tail.

These specimens are somewhat darker on the upper surface of the wings than is the type, but this may be due to the fact that the latter is in very fresh plumage and has more extensive pale margins to these feathers.

Ploceus baglafecht baglafecht (Daudin)

Loxia baglafecht Daudin, *in* Buffon, Hist. Nat. (Didot's ed.), Quadrupeds 14: 245. 1799 (actually 1802) (Abyssinia).

Riney collected a male, 40 km north of Asmara, 2,390 meters elevation, on January 9, 1943. This weaver is known to occur at altitudes of from 5,000 to 12,000 feet in Bogosland and Ethiopia.

Uraeginthus bengalus bengalus (Linnaeus)

Fringilla bengala Linnaeus, Syst. Nat., ed. 12, 1: 323. 1766 ("Bengal"; Senegal substituted by Sclater, Syst. Avium Ethiop. 2: 804. 1930).

One male and one female were taken by Riney on January 1, 1943 in low acacia-dotted open grassy country between Decamera and the Adi-Ugri Road, 2,000 meters elevation. The male had the testes enlarged. Both birds are in worn plumage.

I can see little advantage in accepting the recent suggestion that *bengalus* and its races are conspecific with the *angolensis* group. There is more to be said for Delacour's action in "lumping" *Uraeginthus* in the genus *Estrilda*, but even in this I hesitate to follow him as the cordon-bleus are a very distinctive section, at least, of the waxbill aggregate.

Family FRINGILLIDAE: Finches, Sparrows, and Buntings

Poliospiza tristriata tristriata (Rüppell)

Serinus tristriatus Rüppell, Neue Wirbelth., Vög.: 97, pl. 35, fig. 2. 1840 (Taranta Pass, Abyssinia).

Riney collected a female in the Bermuda grass at the base of the Asmara Plateau, at 1,930 meters elevation 20 km south-southwest of Asmara, January 1, 1943. This seedeater appears to range from altitudes of from 4,000 to 11,000 feet, and is reported (in literature) to be common in Eritrea and northern Ethiopia.

Fringillaria tahapisi septemstriata (Rüppell)

Emberiza septemstriata Rüppell, Neue Wirbelth., Vög.: 86, pl. 30. 1840 (Gondar, Abyssinia).

A male in fairly worn plumage was taken by Riney on January 1, 1943, 45 km from Asmara, 1,930 meters elevation.

This race of this rock bunting is characterized by the extensive rufescent area on the basal portion of the inner web of the outermost primary.

ZOOLOGY.—*Some echinoderms from Biak, Schouten Islands.*[1] AUSTIN H. CLARK and FREDERICK M. BAYER, U. S. National Museum.

Biak (or Wiak) is the largest and most easterly of the Schouten Islands, a small group of islands just north of Geelvink Bay, New Guinea, in approximately latitude 1°S. It is 45 miles long and 23 miles wide. It is a low island, not rising more than 50 feet above sea level except at the southern end where there is a hill 800 feet in height.

Until the late war Biak was regarded as of little importance. It was remote from the Netherlands East Indies capital of Batavia, and the natives, addicted to headhunting and other unpleasant practices, were hostile to strangers. Few collections of any kind had ever been made there, and no echinoderms have ever been recorded from the island.

During the war the junior author, then a member of a 5th Airforce Photo Reconnaisance Squadron, was fortunate enough to spend four months at Sarido village within a stone's throw of a luxuriant coral reef. This reef is a fringing reef of rather insignificant proportions. It is perhaps 200 yards wide at the most and approaches quite close to the shore, which is sandy in the small embayment in which Sarido stands, and rocky southward toward the point on which the airstrip is located. In the little bay the actively growing reef is somewhat farther from shore than elsewhere and is separated from it by a strip of slightly deeper water, from 3 to 5 or 6 feet deep at low tide, with a sandy bottom on which are scattered living and dead clumps of coral and patches of eelgrass and algae. This is a rather restricted zone, grading rapidly into the active reef zone.

The reef itself is covered with water at all but the very lowest tides when a few of the highest coral prominences are exposed. It is made up of great masses of living coral, huge rounded heads of massive species many feet thick and submarine "brier patches" of branched madrepores in which reef fishes of indescribable colors hide by day. It is not an algal reef of the type found in the Marshall Islands and generally throughout the Pacific. There is no prominent ridge of *Lithothamnion*, nor are these plants even present in noticeable numbers.

The coral masses afford precarious footholds, for the delicate *Acroporas* and other branched species are liable to give way, plunging one's foot into 4 or 5 feet of water and raking one's legs with razor-sharp edges in the descent. Among these coral masses are pools of deep water with sandy bottoms, providing a fine habitat for marine animals.

Two invertebrates especially force themselves on the attention of the collector, the giant clam, *Tridacna*, with its mantle rich shades of blue, green, and purple, and a starfish, *Linckia laevigata*, with a very small disk and long, rigid, cylindrical arms of the most outlandish blue imaginable. These two creatures are everywhere. Coral masses are studded with the multicolored zigzags formed by the clams, and the entire reef is dotted here and there on sand and coral alike with the brilliant blue 5-pointed stars.

The most interesting animals were less evident, and it required poking into dark crevices, turning over coral blocks, and stirring up sand pockets to find them. Occasionally one of the giant slate-pencil urchins, *Heterocentrotus mammillatus*, would be found in the open, though usually they were tucked away in remote nooks and crannies, their presence betrayed only by a stray spine or two projecting from a small opening, apparently much too small to allow the creature free passage. Cidarids could be found in similiar situations, though they were much less common.

Turning over coral heads was a simple way of finding interesting things. Brittle-stars were, of course, under every one, and those with large enough recesses often contained comatulids of unusual beauty. The less conspicuous starfishes also were often found in such situations. The little sand pockets under the rocks produced very interesting mollusks, such as the venomous cone shells, *Conus textile*, *C. striatus*, and *C. geographus*, not to mention dozens of less conspicuous things such as abalones (*Halio-*

[1] Published by permission of the Secretary of the Smithsonian Institution. Received November 7, 1947.

tis), several species of *Trochus*, *Mitra*, *Cymatium*, and many other genera.

The large starfishes *Culcita novaeguineae* and *Acanthaster planci* were found crawling about in the open, but because of their protective coloration were more often overlooked than seen. The latter is adorned with the most vicious 3-cornered spines imaginable. In collecting one of these the spines drew blood through a pair of heavy leather gloves.

Toward the outer reef the water deepens, the coral heads become more widely spaced, and, reaching the brink, one can look down the almost vertical wall into fathomless blue. Sharks were sometimes seen cruising along in the hazy middle distance, and always myriads of reeffishes, moorish idols, parrotfishes, pomacentrids, wrasses, scorpionfishes, and many others. To the right and left on almost every coral pinnacle could be seen a huge black or deep red comatulid, gently swaying in the aquatic breezes. These seemed never to stray from their own chosen perch, for on several visits to the same spot we found them unchanged.

The occurrence of *Heterocentrotus mammillatus* at Biak is especially interesting, for only *H. trigonarius* has been definitely recorded from nearby New Guinea. *Heterocentrotus trigonarius* occurs in the Philippines and generally throughout the Pacific islands. It is replaced by *H. mammillatus* in the Hawaiian, Bonin, and Riu Kiu islands, which is also found at Lord Howe Island, in the Murray Islands at the northern end of the Great Barrier reef, and at Cape Jaubert, Western Australia.

The specimens listed below are in the U. S. National Museum, and the numbers following the names are those in the catalogue of the Division of Echinoderms.

CRINOIDEA

Comantheria briareus (Bell), 4, E.6935, E.6937, E.6939, E.6961.
Comanthus bennetti (J. Müller), 4, E.6963, E.6970, E.6972, E.6976.
Comanthus timorensis (J. Müller), 5, E.6936, E.6954, E.6962, E.6874.
Comanthus parvicirra (J. Müller), 5, E.6841, E.6842, E.6966, E.6971.
Himerometra magnipinna (A. H. Clark), 7, E.6957, E.6958, E.6964, E.6965, E.6973.
Stephanometra spicata (P. H. Carpenter), 1, E.6939.
Stephanometra protectus (Lütken), 1, E.6843.
Lamprometra palmata palmata (J. Müller), 4, E.6959, E.6960, E.6969, E.6987.

ECHINOIDEA

Plococidaris verticillata (Lamarck), 1, E.6967.
Eucidaris metularia (Lamarck), 3, E.6968.
Mespilia globulus (Linné), 1, E.6951.
Heterocentrotus mammillatus (Linné), 1, E.6977.

ASTEROIDEA

Archaster typicus Müller and Troschel, 1, E.6980.
Protoreaster nodosus (Linné), 1, E.6830.
Culcita novaeguineae Müller and Troschel, 3, E.6979.
Gomophia aegyptica Gray, 1, E.6828.
Nardoa mollis de Loriol, 2, E.6981.
Linckia laevigata (Linné), 4, E.6982–E.6984.
Linckia multifora (Lamarck), 3, E.6986.
Linckia guildingii Gray, 1, E.6985.
Asterope carinifera (Gray), 1, E.6827.
Asterina cepheus (Müller and Troschel), 1, E.6831.
Othilia luzonica Gray, 1, E.6978.
Acanthaster planci (Linné), 2, E.6955, E.6956.

OPHIUROIDEA

Ophiodera brevispina (von Martens), 1, E.6950.
Ophiothrix longipeda (Lamarck), 1, E.6944.
Ophiocoma erinaceus (Müller and Troschel), 1, E.6952.
Ophiocoma scolopendrina (Lamarck), 2, E.6953.
Ophiomastix annulosa (Lamarck), 2, E.6949.
Ophiomastix lütkenii Pfeffer, 2, E.6948.
Ophiarthrum pictum (Müller and Troschel), 5, E.6946, E.6947.
Ophiarachna incrassata (Lamarck), 1, E.6943.
Ophiarachnella septemspinosa (Müller and Troschel), 1, E.6945.
Ophiolepis superba H. L. Clark, 5, E.6940.
Ophiolepis cincta Müller and Troschel, 2, E.6942.
Ophioplocus imbricatus (Müller and Troschel), 5, E.6941.